NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

VOL. V.

JULY—DECEMBER, 1894.

MACMILLAN & CO.,
LONDON AND NEW YORK.

LONDON

PRINTED BY RAIT, HENDERSON & CO., LTD.,

22 ST. ANDREW STREET, E.C.

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An Asterisk (*) indicates that a figure is given.

NATURAL SCIENCE:

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No. 29. Vol. V. JULY 1894.

NOTES AND COMMENTS.

Mathematical Biology.

In one of his delightful "Imaginary Portraits," Mr. Walter Pater gives a rendering of the mediæval legend of the return of the golden age, as a pagan Renaissance caused by the actual return among men of a denizen of ancient Greece. There came, under the influence of the reborn pagan, a period strangely in dissonance with the times before and after. "One man engaged with another in talk in the market-place; a new influence came forth at the contact. Another and then another adhered. At last a new spirit was abroad everywhere. The hot nights were noisy with swarming troops of dishevelled women and youths with red-stained limbs and faces, carrying their lighted torches over the vine-clad hills, or rushing down the streets, to the horror of timid watchers, towards the cool spaces by the river. A shrill music, a laughter at all things, was everywhere."

It is not the kind of pagan to cause these occurrences who has been born among us. But the prevailing application of physical reasoning to Biology, so often referred to in Natural Science, might be pictured under the idea that someone who, with Democritus, had sat at the feet of Leucippus, had been working in the German laboratories and had been indoctrinating Oxford and Cambridge and London. Professor Weismann's germ-plasm in its later elaborate form is a notable instance of the new tendency. In it the whole organism is formed of units of different grades. The unit of each higher grade is a multiple of the unit of the grade next lower. The multiples of each unit are built up in some physical scheme of architectonics. Bütschli refers the structure of protoplasm and its physical properties to questions of density and surface tension. In many parts of his argument one must have a text-book of physics open to understand him. A large part of the current work upon variation is expressed in mathematical terms, while the study of growth almost necessarily involves mathematical reasoning.

In Dr. Haacke's "Gestaltung und Vererbung," a notice of which appears in another part of our columns, almost a burlesque of this tendency to mathematical reasoning appears. Again and again consecutive pages are filled with formulæ that stretch over three lines of serried signs and figures and brackets, while his new theory of heredity and development depends upon geometrical conceptions.

These physical views are to be traced chiefly in the work of morphologists, and more than once contributors to our columns have contrasted, in this respect, morphological work with the tendency towards vitalism of the physiological school of the present time. But the contrast is more apparent than real. The "vitalists" are studying the phenomena of life as things-in-themselves, as being kindred to, but not compounded of, chemical and physical phenomena. The result of physical reasoning applied by morphologists as yet has been to give a fuller idea of the individual and homogeneous nature of living matter all through the animal and plant kingdoms, rather than to explain the nature of living matter on physical principles. The new method tends not to make biology a branch of geometry, but to introduce a new science of biometry.

Another Critic of Weismann and Another Theory.

In addition to Dr. Haacke's book, we have before us two pamphlets published at Hamburg in 1894, but originally delivered as addresses, by Dr. George Pfeffer, to the Naturwissenschaftlicher Verein. The title of the first, rendered into English, is "The Intrinsic Errors of Weismann's Germ-Plasm Theory." It objects to the theory chiefly on the ground that it implies the conception that Natural Selection works in the same method as would be employed by a creator working with a definite object in view. It is certainly the case that many organisms have structures or properties by which they are saved from destruction or gain an advantage in the struggle for existence. Dr. Pfeffer thinks, and, it is true, many critics of Weismann agree, that Weismann's view of evolution implies an illogical extension of the above statement into the statement that these adaptations, protective or aggressive, have been called into existence by Natural Selection because of their utility; that, in fact, Natural Selection, on Weismann's showing, is a teleological cause. Of course, Weismann himself has expended much pains in attempting to convince, and has succeeded in convincing many, that his theory implies no such illogicality. We do not think that Dr. Pfeffer has brought forth anything that will increase the faith of the opponents of Weismann or unsettle that of his supporters. The title of the other pamphlet is more difficult to render into English. As near as may be it is "The Transformation of Species—a Process of Self-Development." It is based on addresses delivered in March, 1893, and in January, 1894. Like Dr. Haacke, he makes much of the "blessed word" equilibrium. The whole organic world is in equilibrium; a

change of the conditions anywhere throws the whole system out of gear, and when it settles again into a new position of equilibrium, everything will be slightly altered, some things much altered. Similarly, the body of every organism is in equilibrium and changes of any part are reflected over every part. The method of change favoured by Dr. Pfeffer is change of function. Changes of species, he says, occur by reason of the change of function of the species in question.

The difficulty about these, and many similar pamphlets in English and in German, is that they contain much shrewd criticism and ingenious suggestion, coupled with a complete failure to understand the difficulties of the general questions they lightly solve in a magazine article or an after-dinner speech. What we want is not general theories, but particular facts. We want to be shown in the laboratory or in the field how far and in what way changes of function, and so forth, do occur; how the changes affect, and to what extent they affect other parts of the organism; and, above all, how far and in what way these changes are inherited by the offspring of the animals or plants in question. It is the fashion to abuse Professor Weismann's views as theoretical and divorced from experiment and observation, but that is only a fashion of the ignorant. If his conclusions come to nothing, there will be left behind a large and valuable body of contributions to anatomy, embryology, and natural history. Without this side of his work, it is certain that the theory known as Weismannism would have received neither the vast body of praise nor of abuse that have been its meed.

The Normal Nature of Ammonite Abnormalities.

If Weismannism has done nothing else, it has served to instil fresh life into the study of abnormalities and variations of all kinds. This study is beset with many difficulties, and not the least of them is the absence of a standard of comparison. In the investigation of normal forms the observations of one investigator can be confirmed and corrected by those of others. In the case of abnormalities there is much more room for the error arising from personal equation, but where independent investigations lead authors to similar conclusions, it speaks well for the results. Curiously enough, this is to a certain extent the case in reference to a paper which appeared in our last number, "Can the Sexes in Ammonites be Distinguished?", by Messrs. Buckman and Bather (p. 427). After that had gone to press a paper "Ueber Ammonoiden mit 'anormaler' Wohnkammer," was received from Dr. J. F. Pompeckj.[1] It deals with Ammonites which are said to show "abnormal body-chambers," that is to say, body-chambers which in breadth or thickness are less than they should be if the previous rate of increase of the shell had been maintained.

[1] Jahreshefte Vereins Naturkunde Württ. 1894. Pp. 220–290, pl. iv.

The opinion of Munier Chalmas that this so-called abnormality was a sexual character was commented on adversely by Messrs. Buckman and Bather, who regarded it as one of the characters of old age, and we now note that Dr. Pompeckj comes to a similar conclusion. His paper also deals with the question of resorption or absorption of the body-chamber in course of growth. This idea was put forward to explain the occurrence at the same localities of very different-sized Ammonites of presumably the same species, the smaller with "abnormal" body-chambers, and the larger (the presumed adults of the smaller) with normal coiling at the diameter of the smaller. Dr. Pompeckj, however, rejects such absorption as a principle of Ammonite growth, and explains this concurrence of forms by Dr. Walther's doctrine of dispersal after death (Nat. Sci., iv., p. 245), intimating that all such specimens, whether big or little, are full grown. But there are other difficulties in connection with this, which Dr. Pompeckj's hypothesis does not seem to meet; and his conclusion (p. 289) that "an Ammonite with abnormal body-chambers is, almost without exception, to be considered as full grown," may not be readily accepted. Other opinions of Dr Pompeckj's will no doubt be challenged, and exception might be taken to the application of the term "abnormal" to that which is a normal process of growth in racial or individual decline, and which even when carried to excess, as in *Macroscaphites*, scarcely deserves to be called an abnormality. All the same, the work is a notable addition to Ammonite literature.

Autotomy in Echinoderms.

The obscure processes of budding and regeneration are at least as interesting as the nature of variations. It is a well-known text-book fact that Echinoderms have considerable power of recuperating external injuries, to such an extent, indeed, that a single arm torn off a starfish is said to reproduce the whole animal. It is also generally believed, and in some cases known to be true, that these animals can of their own accord break off or eject portions of their body, which may, in a few instances, form new individuals. We wish to direct attention to two observations, which, though not very recent, seem, owing to the places of their publication, not to have attracted the attention they deserve.

In the fifth volume of the *Proceedings of the Liverpool Biological Society* (p. 81), Mr. H. C. Chadwick describes how he placed three individuals of the holothurian, *Cucumaria planci*, in a jar, and how a strange thing happened to each of them. The middle portion of the body was pulled out and became very thin, till at last the animals broke in two. Then the front halves crawled away, "leaving their tails behind them" motionless. Each front half developed a new anus, and each hinder part developed a new mouth and circlet of tentacles. Then one of the animals derived from a hinder part divided again,

so that before two months were up Mr. Chadwick had seven sea-cucumbers instead of three.

In the twenty-first volume of the Norwegian North-Atlantic Expedition, Dr. D. C. Danielssen tells us that among the specimens of *Bathycrinus carpenteri* dredged, a large number were found to have thrown off the part of the crown that lay above the basals, and in several instances to have reproduced it again. He further points out that, while the other specimens dredged had genital products developed in their pinnules according to the normal method of sexual reproduction, the crowns that were thus thrown off were sterile; and he suggests that, when the sexual powers of the old crown are worn out, it is rejected, and a new one with fresh powers grown in its place. Now, if we remember that the most recent investigators concur in deriving the genital rachis of the crinoid arm from an extension of the so-called "dorsal organ," which lies between the basals, then we see that this recuperation of the crown may be regarded as a means of obtaining a fresh extension of this dorsal organ into that part of the crinoid where alone its generative faculty is available for sexual purposes—namely, into the pinnules. This idea certainly countenances Dr. Danielssen's suggestion. At the same time, we must not forget that in certain ophiurids, as Lütken and Cuenot have observed, reproduction by fission only occurs in immature individuals, and sexual reproduction appears to ensue only when this asexual method has been many times repeated. On the other hand, it is interesting to observe that, out of Mr. Chadwick's three holothurians, two discharged a quantity of ova before fission set in. Another interesting fact may be gathered from Dr. Danielssen's account, to wit, that the stages of growth of the new crown are similar to the ordinary stages of growth in the young crinoid: the arms long remain closed, and the anus is formed at a late period. Until the alimentary canal resumes its functions, whence does the rapidly-growing animal obtain its nutriment? What can be its reserve store? Is it indebted to the dorsal organ, and is this regeneration more closely allied to development *ab ovo* than we think; or does the animal live on the blood contained in the cavities of the stem?

Few observations have yet been made on the relations of spontaneous fission to sexual reproduction in the Echinoderms, and the subject seems to afford a fertile field for investigation, not without its bearing on the deeper problems of the time.

The Depths of the Sea.

Increased knowledge of the depths of the sea will no doubt help us to a solution of problems like those we were discussing in the foregoing paragraph; but, notwithstanding the numerous exploring voyages of the past 20 years, we have still much to learn about this part of our globe.

Dr. Otto Pettersson has just published, in the *Scottish Geographical*

Magazine for June, an important paper on Swedish Hydrographic Research in the Baltic and the North Seas. Dr. Pettersson commences by stating that "the system adopted by the Swedish naturalists at the present time, in their explorations of the sea which surrounds the Scandinavian peninsula, is to despatch a number of ships simultaneously from different ports across that part of the sea which is to be explored, each ship being provided with a complete set of hydrographic instruments, worked by assistants specially trained for their task in the laboratories of Stockholm's Högskola, the Polytechnic Institute, etc. The route of each ship, and the position of every sounding station, are determined beforehand, according to previous experience. In every successive expedition the same sounding-places are chosen, in order to ascertain the alterations which have occurred in the arrangement of the water-strata. The advantages of this method of research are obvious. All the observations being taken within a few days, the hydrographic state of a certain part of the sea at all points is exhibited practically simultaneously, unaffected by changes of wind and weather." Surveys of the Baltic have been undertaken in 1877, 1878, and 1879, but the more systematic surveys of which this paper treats were taken between 1890 and 1893.

After treating in detail, and with illustrations, of the special apparatus used, Pettersson gives specimens of the results obtained as regards temperature at various depths in the Gullmar Fjord, the deepest depression of the Baltic (S.S.E. from Landsort), and the deepest part of the Aaland Sea. In the Gullmar Fjord temperature ranged steadily downward from 17·50 C. to 4·60 C. (60 metres), then rose again gradually to 5·04 C. (120 metres, bottom). In the Landsort series the temperature marked 13·06 C. at the surface, 2·15 C. (60 metres), rose again gradually to 4 C. at 280 metres, to sink again to 3·95 C. at bottom (400 metres). In the Aaland Sea 8·05 C. was registered at the surface, 1·80 at 30 metres, 2·95 from 40–70 metres, and then a steady fall to 1·65 at 265 metres (bottom). Full instructions are given for the method of obtaining analyses of gases dissolved, and the determination of the total carbonic acid contained in sea-water, as well as chlorine titrations, specific gravity, and salinity. A description is then given of Pettersson's Plankton apparatus, the results obtained with which showed that water-layers of different origin and different depths contained different and characteristic forms of animal life. For instance, on 2nd August, 1893, the biologists accompanying the expedition distinguished three strata of water in the Gullmar Fjord, which showed marked differences with regard to temperature and salinity, and marked differences in the fauna. The detailed results are given in the pages, but space precludes our inserting them here. Dr. Aurivillius is now engaged on an investigation of the animals obtained, and we await his results with interest. The final paragraph of this interesting paper is

as follows:—"The Plankton obtained in November was very different from that which we found at the same place and depths six months before. Suffice it to say, that in quantity the winter Plankton was far in excess of that of the summer; that the vegetable matter seems to predominate in the colder season; and that abundance of living organisms were brought up from a depth of 70 metres in November, while no Plankton was found at the same depths in August. In the middle of the Skagerack the character of the Plankton at 30 and 40 metres is different from that at 10 metres, a fact which could be predicted from the hydrographic conditions of the central part of the Skagerack."

The "Quarterly Review."

The Swedish explorations of the ocean floor lead us naturally to the ocean surface. The current number of the *Quarterly Review* has an intelligent article entitled "Ocean Meadows," in which is given an interesting popular account of surface flora and fauna. The account is based largely upon the revision of those wide-spread algæ, the heterocystic Nostocaceæ, by MM. Ed. Bornet and Ch. Flahault, and on Maurice Gomont's monographs on the Oscillaria. The writer points out the many problems of pelagic life still waiting solution—problems that have not only a scientific but a great economic interest. For without question the "unvintagable sea" is one of the greatest food sources of the land. The fisheries of the world are capable of almost indefinite extension: but this extension can proceed only when we have sufficient knowledge of the feeding and breeding of food-fish to allow us to take full benefit of this supply of food without disturbing breeding grounds or driving fish to remote inaccessible haunts. The writer urges that further work be done. "No costly equipment is needed. The use of a cruiser (of dignified speed only) would, no doubt, be furnished by the Admiralty for a brief period, while the Government grant administered by the Royal Society is often spent with less return than an investigation of this kind, costing a small portion of its annual amount, would yield. Let the fitting men come forward and demand it."

Whether or no the precise method recommended by the "Quarterly Reviewer" be the most excellent, we rejoice to see a claim long advocated in scientific journals so ably enforced by a journal of which none can say that it is prejudiced by too strong a predilection for things scientific. The writer of the *Quarterly Review* article might have strengthened his case by reference to two elaborate monographs dealing with oceanic matters, and both recently noticed in Natural Science. The results published by Dr. Brooks in his Monograph on Salpa (*see* Natural Science, vol. iii., pp. 222–225, and vol. iv., pp. 457–462) bear in many important ways upon economic problems. Dr. Brooks was aided by his Government, for

the material was collected by the United States Fish Commission. The beautiful monograph by Mr. Savile Kent on the Great Barrier Reef (*see* NATURAL SCIENCE, vol. ii., pp. 453–460) is of at least equal economic importance, and serves the better to point the present moral, in that the expense of the exploration and of the publication of the volume was largely borne by the Government of Queensland.

Another article in the same issue of the *Quarterly* is of greater length and equal scientific interest. It makes a demand, not upon the pockets of the ratepayers, but on the fortitude with which they shall see the breaking of an old idol. It has been one of the most popular of our beliefs that Shakespeare was as faithful a depicter of birds and beasts as he was of human nature. But the generation of critics who have palmed upon us this belief have themselves been no naturalists. In this article the "natural history" of Shakespeare is shown to consist almost entirely of literary conventions. In the majority of cases, instead of observing for himself, he has been content to borrow uncritically from his predecessors.

THE JUNGLE BOOK.

ANOTHER writer, who, if he be not read two hundred years after this as much as Shakespeare is read to-day, yet is read more to-day than was Shakespeare by his contemporaries, has written much of animals. But, in addition to the fact that discussing the "Nature writing" of our greatest English writer leads us to the "Nature writing" of our own days, we make no apology for directing the attention of all naturalists who have still that good fortune before them to Mr. Rudyard Kipling's new book. It is an old and not unfounded reproach that too close a scrutiny of the mechanism of the organic world tends to destroy for anatomists the romance of the living world. For them too often Nature becomes an explicable or partially explicable mechanism and an animal a bundle of tissues to be dissected, or the mere seat of functions to be explored and measured. A great novelist once said that the best novels were Nature seen through a temperament. Here is a book about animals seen through a temperament, very different from that generally in association with biological investigation. It is written in the spirit of primitive races, to whom the beasts and the birds of the field and the forest are a people, with their own language and customs, for whom the woods and the plains are full of the mysterious glamour of life. The book tells of the battles and the alliances of animals, of the noon-day heat of the plains, and the stealthy bustle of the night. Keen observation and the artistic method employed by Mr. Kipling in his stories of man do not fail him in these stories of animals, nor is there wanting evidence of that peculiar turn of mind by which Mr. Kipling sees in the canons of army life the supreme standard of moral excellence. But criticism, as the author himself would say, "is another story." Of the tales in the "Jungle Book," two in especial

delighted us. In an earlier volume, under the title "In the Rukh," Mr. Kipling described how an Englishman, serving under the Commissioners of Woods and Forests in India, found an ideal native ranger. This was the foster-brother of wolves who, having grown up among the beasts of the jungle, had learned all their secrets and had been sworn into alliance with them. Here is the story of his up-bringing: of how he was taught by Baloo, the bear, and Bagheera, the black leopard, and of how he fought and conquered Shere Khan, the man-eating tiger.

The other story that attracted us is laid in very different surroundings. It tells of the lives of seals, of their breeding-places, and of their molestation by man.

In this volume, where animals are personified and made to tell their own stories, it is of no value to trace out the exact degree to which zoological accuracy has been carried. In the main the characters of the animals are displayed so that they need not disturb the equanimity of the most sensitive taxonomist. The general reader will get a picture of the chief animals dealt with, such as all the details of the taxonomist would never give him.

The Education of Sally.

Mr. Rudyard Kipling has written several stories of monkeys, but we doubt if he has studied them so carefully as the late Professor Romanes, who successfully conducted the education of the chimpanzee at the Zoological Society's Gardens. Although a full account of this was published in the *Proceedings* (1889, p. 316) of the Society, our readers may find it interesting to have the matter recalled. Sally's education was carried on under considerable difficulties, as the constant presence of visitors distracted her attention, and required an unusual amount of patience in her teacher. She was first taught to associate actual numbers with the names for them. This was done by asking her repeatedly for one straw, two straws, or three straws; these she was to pick up and hand out from among the litter in her cage. When she handed a number not asked for her offer was refused. When she obeyed correctly she was rewarded by a piece of fruit. Lastly, if two straws or three straws were demanded, she was taught to hold one straw or two straws in her mouth until she had picked up the remaining straw, and then to hand the two or the three straws together. This prevented her from being correct in her action merely by the interpretation of vocal tones.

Before long she learned exactly what was meant by the sounds of the first three numbers, and then with similar success her education was extended up to six. Beyond six she was inaccurate, but she never failed to realise that seven, eight, nine, or ten meant a number of straws greater than the lower numbers.

Professor Romanes believed that the result of these experiments

did not imply that in "counting" the ape employed any system of notation. "We know," said he, "from our own experience, that there is counting and counting, *i.e.*, distinguishing between low numbers by directly appreciating the difference between two quantities of sensuous perc eption, and distinguishing between numbers of any amount by markin g each perception with a separate sign." In the case of some persons, he went on to say, the former method of perception could reach so far as the number twenty. It was this method that he imagined the ape to exhibit, and as the perceptions of such an animal are naturally less proficient than those of man, it was natural that above six or seven vagueness should begin, while, at least, it was perceived definitely the number was greater than those correctly interpreted.

Professor Romanes went on to attempt to make Sally associate colours with their names, but although Sally could distinguish between white and coloured straws, she could not be taught to distinguish between any of the colours. Hence he came to the remarkable conclusion that Sally was colour-blind, although of course he had no means of deciding whether her colour-blindness were only an individual defect.

The Bird's Foot.

In Mr. Finn's interesting paper on the bird's foot, published in our last number (Natural Science, vol. iv., p. 453), the writer assumes, and most ornithologists would agree, that the typical number of digits in the bird's foot is four. In support of this he refers to Forbes's discovery of the rudiment of a hallux in three-toed birds like the albatross. But he did not mention an important piece of evidence discovered, or at least noted, by Garrod, and pointing in the same direction. The chief muscle of the hallux is a flexing muscle named the *flexor longus hallucis*. In all cases where the hallux is absent, this muscle still persists with a well-developed fleshy belly and a separate tendon, the latter uniting with the deep flexor tendon of the other toes. This, certainly, is strong evidence, were more evidence wanted in favour of three-toed birds being derived from a four-toed stock. But viewed merely as a rudimentary organ, it is not so interesting as at first appears; for in the great majority of birds, and in all near allies of three-toed birds, the tendon of the flexor of the great toe is connected by a tendinous slip with the common deep tendon of the other toes. So it must be inferred that the *flexor longus hallucis* of three-toed birds remains, not as a mere rudiment, but because it is functional, as an accessory to the flexor of the other toes. A three-toed passerine bird would be interesting, because in passerines there is no connection between the flexor of the great toe and the common flexor of the other toes. But there is no three-toed passerine. V. p. 159

The Colours of Birds' Eggs.

The value to be assigned to the colour of eggs in classification, is a point in which the personal equation plays an enormous part. Probably no two ornithologists would agree about many of the most common cases. Some of the difficulty, no doubt, is due to our ignorance of the natural processes by which the colours and markings are produced. Dr. Heinrich Wickmann has recently published, at Münster, a pamphlet of sixty-four pages on the origin of the colouring of birds' eggs. He states that the egg-shells of most birds consist of three parts. There is the inner elastic and fibrous layer, with which we are all familiar, and which is double, the middle calcified layer, and a thin organic cuticle, which is not found in all eggs. In coloured eggs the ground colour is to be distinguished from flecks of colour. These latter extend only a very small distance into the calcified layer, while the ground colour penetrates to the innermost part of the shell. But white eggs, says this author, are not unpigmented and white merely because chalk is white. There are separate white pigments present, and an egg of which the white ground colour is due to one pigment may be flecked with white spots of another white pigment.

He agrees with Kutter that the colours are deposited while the egg is in the uterus, before it has reached the cloaca, and gives a list of birds, including the duck, the heron, partridge, crow, falcon, blackcap, cuckoo, nightingale, and snipe, in which he has found actually in the uterus an egg with the complete normal colouring. Further, he rejects all former hypotheses as to the place of origin of the colours, and comes to the conclusion, although on what seems to us insufficient evidence, that all pigments are formed in the ovary shortly after the egg has entered the tube, and are discharged into the tube, down which they follow the egg to the uterus. He believes that the origin of the colouring matters is to be found in the blood, which undergoes katabolic changes during the formation of the *corpus luteum*. The different colours of the eggs, even of nearly-allied birds, he explains in a somewhat cavalier manner, by attributing them to slight differences in the chemical constituents of the blood of these animals. We do not think that Dr. Wickmann has left nothing for future investigators, and we regret that he has given insufficient details of his methods of investigation; but he has made a notable contribution to oölogy. In a concluding paragraph, the writer says, with some pride, that all the work was done in his private room, and that he has to thank no professor nor institution for aid.

Tadpoles.

The day has gone by for accepting Tadpoles, or indeed any larval forms, as simple survivals of an ancestral condition. We know now that the larvæ of many sponges echinoderms, marine

worms, and above all of many insects, are so modified in connection with their own conditions of life, that only in the broadest way can they be said to be more primitive than the adult forms to which they give rise. They are more primitive chiefly in that they are simpler animals, animals with less complexity of structure, and thus animals which have a broad similarity to the less complicated more ancient form of life. In the case of the larvæ of those animals which are now called *Chordata* because, though they may never attain to the possession of a bony backbone, they possess the gelatinous notochord which is accepted as the predecessor of the backbone, there is strong reason to believe that the simple structure of the free-swimming stage is misleading. The Tadpoles of Ascidians and Amphibia, for instance, possess many simple characters which we believe that the original ancestors of all the Vertebrata possessed. But this simplicity is most probably a case of convergence. Tadpoles live in water. They capture their food, escape from enemies, move about, live and breathe much in the same way and in the same surroundings as the simple pelagic ancestors of the Vertebrata. And thus in many ways they resemble these ancestors. But they are not now taken, at least by most zoologists, to be actual surviving ancestral stages.

The curious Tadpoles of the African Frog, *Xenopus lævis* (*Dactylethra capensis*), may be taken as a favourable instance of this change of view. Among the striking peculiarities of these Tadpoles are a development of pigment so slight that the nerves and blood vessels can be seen through the transparent skin; a pair of long whip-like tentacles protruding in front from the angles of the mouth, and a fish-like appearance, due to the absence of any constriction between the broad head and the body. These points, and some others, were taken to imply that the Tadpoles of *Xenopus* represented in a peculiarly intimate fashion some fish-like ancestor of the frogs. This resemblance was exaggerated in some figures published by the late W. K. Parker in an otherwise most valuable account of this Tadpole. The figures were copied in many text-books, and the fish-like appearance was still more exaggerated, some innocent markings on the back of the animal being so drawn as to resemble the dorsal plates of some extinct armoured fish.

Mr. F. E. Beddard, F.R.S., recently had an opportunity of re-investigating these forms, and the results of his work appear in the last issue of the Zoological Society's *Proceedings* (1894, p. 100). He shows how some of the mistaken impressions have arisen, and describes how, in most respects, the Tadpoles of *Xenopus* are not markedly different from the Tadpoles of the common Frog. In the first two days of their life they possess, just like the frog, a suctorial gland below the mouth, and the head is separated from the body by a constriction. After the third day these disappear, and the peculiarities of the more advanced form come into existence. The details of structure mentioned are too technical to repeat here, but the

general conclusion of Mr. Beddard's investigation is that there is no more reason to consider the Tadpoles of *Xenopus* fish-like and ancestral than in the case of any other Tadpoles.

The Botany of Tropical Africa.

The recently issued number (ser. 2., vol. iv., part 1) of the Linnean Society's *Transactions* (Botany) contains an account of the plants of Mt. Milanji, Nyasaland, collected by Mr. Alexander Whyte. At the instigation of Mr. H. H. Johnston, our Commissioner and Consul-General in British Central Africa, Mr. Whyte, towards the end of the year 1891, explored the Natural History of this mountain and district, and his collection of plants has been worked out by the officers of the botanical department of the British Museum. The region is about twenty miles to the south of Lake Shirwa, and sixty miles W.S.W of Blantyre. It comprises an isolated range of, for the greater part, precipitous mountains, many of the gullies and ravines being well wooded, while favoured nooks of the highlands are bright with gorgeous displays of flowers. Including those now described for the first time, no less than 62 per cent. of the species collected by Mr. Whyte belong to tropical vegetation. Six per cent. of the remainder are plants with a wide distribution in the tropical and subtropical regions of the world. Thirteen per cent. are North African plants, chiefly Abyssinian, while nineteen per cent. are South African. Milanji is, in fact, in the region where the floras of North and South Africa meet and intermingle with the characteristic tropical vegetation. Several Malagasy plants are also found. Of special interest are two species of *Erica*, which carry the South African heaths into the tropics, and a new conifer, of which the nearest allies are also found at the Cape. The plant in question, *Widdringtonia Whytei*, is described as a magnificent tree, reaching a height of 140 feet, sometimes with a clear straight stem for 90 feet, and a diameter of 5½ feet at six feet from the base, sometimes giving off long, straggling branches nearer the base. The timber is of a pale reddish colour, of excellent quality, and easily worked. Unfortunately, these fine trees are rapidly disappearing before the forest fires, the few left being confined to the upper ravines and valleys, a large forest finding a comparatively secure habitat in the damp gorges of the Lutshenya valley. It is pointed out that its nearest ally, *W. juniperoides*, of the Cederberg Mts., Cape Colony, seems also to be dying out, as it is now rare, though it once formed great forests. We are glad to learn that steps have been taken for the preservation of the Milanji forests, and that seedling plants are being raised with the prospect of affording increased supplies of this useful timber in the future.

Apropos of trees, the great hall at the Natural History Museum now boasts the largest section this side the Atlantic. In one of the bays on the right-hand side will be found a horizontal slice from one of the Californian mammoth trees (*Sequoia gigantea*). It measures

15 feet across, and its age, when cut down in 1892, has been computed, by counting the annual rings, at 1335 years, which means that it sprang from the seed in 557 A.D. It is somewhat appalling to think of the ages required for the action of Natural Selection in forms where the life of the individual is prolonged to such an extent.

The Museums' Association.

The Museums' Association may be said, in fashionable phrase, to have arrived, when so eminent an authority as Hofrath Dr. A. B. Meyer, Director of the Royal Zoological and Anthropologico-Ethnographical Museum at Dresden, dedicates to it a handsome quarto, with twenty plates, descriptive of the various improvements in Museum fittings that he has introduced into his own Museum. This volume, which is, by the way, the first number of a new publication, the "Abhandlungen und Berichte," of the above Museum, describes in detail and gives working plans of wall-cases, table-cases, skeleton-stands, large spirit-tanks for exhibition purposes, cases of drawers, cases and trays for eggs, nests and shells, a craniometer and a craniophore, trucks and trollies and other minor aids to Museum exhibition. In the construction of cases, trays, and drawers, Dr. Meyer's chief peculiarity lies in the exclusive use of glass and iron, which render the cases fireproof and, he contends, absolutely proof against dust. A description of some of these cases has already been given by him in the Report of the Museums' Association for 1891, and we hope that some member of that body will lose no time in publishing an English rendering of this very suggestive and practical paper.

The Museums' Association is, at this our time of publication, meeting in Dublin, and the address of the President, with which, through the courtesy of Dr. Ball, we are in this number able to present our readers, deals with the history of the Dublin Museums. Meanwhile, there has just been published in the *Proceedings of the Liverpool Naturalists' Field Club* a very similar address by their President, Mr. G. H. Morton, which relates the story of the Museums of Liverpool. The present Museum, which has reached a very high stage of development, through the exertions of the late Rev. H. H. Higgins and Mr. T. J. Moore, was preceded by quite a different Museum, belonging to the Royal Institution of Liverpool. Under the care of William Swainson, T. Stewart Traill, Henry Johnson, Francis Archer, and other well-known naturalists, the collections of this Museum assumed considerable importance, and included a large number of valuable type-specimens. "It was," says Mr. Morton, "very useful to students of natural history, and a source of instruction to the general public. For many years 30,000 persons visited it annually on the free days." But with the rise of the present Free Public Museum, and after the death of the curator, Henry Johnson, a sad state of things ensued. "Neither the proprietors nor the committee cared anything about the Museum, and it perished from

neglect. In 1877 the Mammals, Reptiles, Crustacea, Polyzoa, and Actinozoa were sold to the Corporation of Nottingham, and soon after the Birds, Mollusca, Fossils, and Minerals to the Corporation of Bootle. The Museum contained thousands of interesting specimens collected by members of families still well-known in the city, and they were presented in the hope that they would be preserved—and many were well worth preserving—but the labels and all particulars have been lost and nothing is remembered about them." Mr. Morton does not draw any moral, but it is plain enough. First, co-operation and not competition is wanted in the scientific world; neither men nor money are enough to support more than a very limited number of scientific institutions, even in one of the richest cities of the world. Secondly, if proprietors and committees are unable to recognise the public and national, or rather international, nature of their trust in the preservation of scientific specimens, then the nation should have power to take their trust from them. The sale of these valuable collections may have been within the limits of the law, but it was none the less an iniquity and morally unjustifiable.

Sunday Opening of Museums.

Before leaving the Museums' Association, we cordially recommend its members to read Mr. Holman Hunt's recent address to the Sunday Society. Dr. Ball himself gives evidence that the Museum Curator, overworked though he be, welcomes the opening of his Museum on Sunday. Otherwise he must feel that his labour is largely wasted, since so many of those for whom it is intended have no chance of seeing it. In provincial Museums, too, where so much of the curator's work must be done in the public galleries, Sunday, the curator's holiday, is pre-eminently the day for the admission of "the great public." Such curators will read with pleasure the following extracts from Mr. Hunt's eloquent address:—

"To Christians afraid of scientific pursuit, and who looked upon art as far from deserving of confidence, he would say a word to dissipate their fears. Museums, it was true, presented startling facts to the eyes of men who, like the Mohammedan conqueror of Egypt, ordered the burning of the Alexandrian library with the verdict that all wisdom necessary for the true believer was in the Koran. Now, independent investigation into the bottomless pit of knowledge must by all scientific students be impartial, and must stop at that point where materialistic facts failed to afford further evidence. They, therefore, gave no intentional corroboration of spiritual ideas. He believed that every full-minded person who went to a Museum, and made himself acquainted with the evidences existing there of the links in the order of creation, and of their relation to earlier and later facts, had instinctively increased in him the certainty of the Author's existence, and of His grandeur and of His all-sufficiency to bring about justice and mercy. One most important lesson taught in all Museums, whether the example looked at be a work of nature or of art, was in the demand it had made for long-continued patience. Every fossil spoke of its adaptability to its conditions when in life, as having been produced

by the exercise of steady overcoming of difficulty, and it marked a gradual step towards higher perfection; and so every work of art—and in this class they might include mechanical contrivance—declared as traceably the patient control of the Maker's energies. . . . The exquisite work of all ages, which all might see in Museums, was the surest spur to ambition, and this in its full force made all pains and patience well spent in the accomplishment of good work, and made the artist of every kind turn with disdain from encouragements to mediocrity, which were certainly among the dangers of our day."

Darwin and the Y.M.C.A.

During the recent assembly of Christian young men in London, the Rev. James Hastings, M.A., of Kineff, N.B., was reported by the *Daily Chronicle* to have made the following remarks:—

"This is the age of specialists and dwarfs. Darwin is the best example, and I refer to him just because his case is so well and widely known. You remember his confession: that he had spent himself so long and so exclusively upon insects, that the avenues of the soul into which God and music were wont to enter had become closed for ever. This seems to be Darwin's contribution to the knowledge of the world, that responsible men may lose their organs if they refuse to use them."

We do not think of accusing the Rev. James Hastings, M.A., of Kineff, N.B., of bearing false witness, for it is plain that that good man has never troubled to look into Darwin's writings for himself; but it is interesting to notice the evolution of this myth, now "so well and widely known." It was founded upon a few lines in the autobiographical part of Darwin's "Life and Letters." The complete passage is too long to quote; anyone may find it in the first volume of the "Life and Letters," pp. 100–102. Darwin says that he lost the taste for music and poetry and pictures which he had up to about the age of thirty, but that novels remained a wonderful relief and pleasure to him, especially if they had a happy ending. He suggests that his mind had become a "machine for grinding general laws out of a large collection of facts," and that this may have caused an atrophy of that part of the brain alone upon which the higher tastes depend. Of course, there was no word about the soul or God, and as for music, one may read a few pages further on that he never had any ear for music, and that although, like many other people with a similarly defective ear, he enjoyed hearing many tunes, he was never able to recognise an old favourite.

It was that astute ecclesiastic, the late Canon Aubrey Moore, who first laid hold of this passage for controversial purposes. He himself knew well, and did much to combat the view, that Darwinism and infidelity were interchangeable terms, but apparently he could not resist the temptation of suggesting that poor Darwin was devoid of a faculty that the Canon and other ecclesiastics possessed. It was left for later and narrower-minded persons to work up the idea into its present form.

I.

Some Shell-boring Algæ.

A NOTE in the February number of NATURAL SCIENCE (1), to the effect that botanists seemed inclined to keep to themselves information on the subject of shell-boring algæ, suggested that it might be of interest to readers if some account of the results of the investigation of these plants by MM. Bornet et Flahault (2) were forthcoming. The gradual disappearance of the empty shells of bivalves and other molluscs has, until recently, been regarded as the result of their disintegration by the friction produced by the movements of the sea-water, and by the slowly dissolving action of the sea-water, charged with carbonic acid. No doubt this explanation holds good in part, especially for localities where the sea is "wild." In quiet, more or less land-locked bays, another, and to the naturalist much more interesting, cause is now known to contribute to the disappearance of the empty molluscan shells. During the last fifty years many naturalists have made known the presence of tubes of various kinds, branching, and penetrating in different directions, into the substance of fossil and recent corals, fish-scales, and molluscan shells. The late Professor M. Duncan (3) gives, in "On some Thallophytes parasitic within recent Madreporaria," a summary of earlier investigations of the subject, and adds considerably to it himself. Unfortunately, the botanical aspect of the question is by all ignored or misunderstood, the tubes are considered to be parasites living on the organic matrix of the shell or coral, and are of interest chiefly as illustrations of the persistence, through long ages, from the Silurian to the present time, of identical or almost identical lowly organisms. Thus the boring tubes are ascribed to "*Saprolegnia* or *Achlya*, a confervan, a fungus with green coloring matter, if indeed, *Saprolegnia* can be considered a fungus." Two species, *Achlya penetrans* and *Achlya* (*Saprolegnia*) *ferax*, Ktz., are recognised by Duncan, but with this reservation:—"The classificatory position of the parasite is in the midst of a group of forms which have complicated life-cycles, such as the Achlyans (proper), the Saprolegniæ, the Empusinæ and Botritidæ, and the filamentous false-root bearing genera *Codium* and *Bryopsis*—forms which are more or less the expression of one organism under different conditions and age." Professor Duncan's investigations were mainly concerned with corals, of which

he examined many recent specimens, from the littoral zone to a depth of 1,095 fathoms, from waters, with a temperature ranging from 39·7° F. to 55° F., from Davis Straits southwards. His general conclusions were that the boring tubes are more abundant in a coral, the warmer the sea-water and the less the pressure to which it is subjected, and that "the fossil corals of Silurian age were also affected by closely allied, if not specifically identical, forms." It remained for the Swedish botanist Lagerheim (4), in 1886, and more especially for the French algologists Bornet and Flahault (2), in 1889, to supply an accurate botanical account of the perforating organisms, and to point out their most important biological significance—that of shell destroyers.

In a paper by Bornet and Flahault, entitled "Sur les algues perforantes," ten genera are described and their life-histories in some cases more or less fully made known. Of these ten, five are new to science, and are confined, so far as is known, to the peculiar habitat furnished by molluscan shells. All the perforating plants agree in their general mode of development. At first they spread out horizontally in the epidermal layer of the shell, forming an irregular network or radiating round a central point. From the horizontal layer branches grow out, some at right angles, penetrating into the substance of the shell, dissolving its calcareous matter as they proceed. Other branches elongate themselves, parallel to the primary filaments. As time proceeds, these horizontal filaments become so numerous and their branches so close together, that the intervening calcareous matter of the shell finally disappears, and the plant, now in immediate contact with the surrounding water, is able to send off its reproductive cells. The surface of the shell becomes at the same time rough and irregular. It is to Bornet and Flahault we owe the important generalisation that this constant gnawing away of the shell ends in its complete destruction, and is the chief cause of the disappearance of shells in quiet bays where they are not mechanically destroyed by the ceaseless activity of the waves.

The boring plants belong mostly to the blue-green algæ (Cyanophyceæ), and the green algæ (Chlorophyceæ); one is a red alga, two are fungi, and the brown algæ are unrepresented.

The presence of a perforating green plant in a shell is easily distinguished from a mere superficial green deposit of germinating *Ulva*, etc., by scraping with one's nail, or, better, by breaking the shell in two. If the broken surface shows greenness to any depth, one may be satisfied it is due to a perforating alga. The identification of the particular species present requires, in most cases, decalcification of the shell and microscopic examination subsequently. A good deal of information can often be obtained by examination of the shell untreated, under the quarter-inch objective, or by rough powdering of the shell in a mortar. Many of the fragments thus produced stand on edge when placed on a slide, and can be examined as shell sections.

The examination of the living plant is, as the French algologists state, extremely troublesome and fragmentary. The most satisfactory method of examination is by decalcification of the shell with Perenyi's fluid.[1] This solution slowly dissolves the calcareous matter of the shell, and fixes the protoplasm of the boring plant, without destroying its colour. The three commonest and most readily recognised plants are *Gomontia polyrhiza*, *Hyella cæspitosa*, and *Mastigocoleus testarum*. *Gomontia* produces distinct green patches, which, seen with a pocket lens, appear as numerous branching green filaments radiating from a common centre. At certain times dark green specks, the organs of reproduction or sporangia, are observable on the filaments. These sporangia, recognisable readily enough, when young, as part of *Gomontia*, acquire such peculiar characters ultimately, that there is every justification for the mistake made by Lagerheim, who discovered them, of describing them as independent plants of the genus *Codiolum*. *Hyella* and *Mastigocoleus* are often found together, and also intermixed with *Gomontia*. When found pure, *Hyella* is distinguished from *Mastigocoleus* by its patches, which are smaller, more numerous, closer together, later confluent into a network which may give the shell a mottled appearance "like a Fritillaria petal." Both genera give patches of the same colour, grey, blue-black, or violet (Payne's grey); but in *Hyella* one cannot make out with a pocket lens such fine radiating filaments as compose the patches in *Mastigocoleus*. The red alga, *Conchocelis rosea*, Batt., was not known until 1892, when Batters (5) obtained it in 6—8 fathoms off the Cumbrae Islands, in the shells of *Mya truncata* and *Solen vagina*. *Conchocelis* is recognisable to the naked eye as a pink stain penetrating into the substance of the shell. Many of the genera, and the two fungi especially, give no external indication to the naked eye of their presence in the shell, and are only recognisable after decalcification and subsequent microscopic examination.

It would be out of place here to enter into a consideration of the numerous interesting botanical questions discussed by Bornet and Flahault. It must suffice to give an adapted form of their table of genera, and the characters by which they are most easily recognised.

A. Coloured Plants:—

1. Rhodophyceæ (Red algæ)
   Bangiaceæ ?

Filaments branched, septate, chromatophores pink and star-shaped, spores (?) single.—*Conchocelis*.

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[1] Perenyi's fluid: Nitric acid, 10 per cent., 4 vol.
Alcohol, 3 vol.
Chromic acid, 0·5 per cent., 3 vol.

This reagent has long been known to zoologists. I used it through their suggestion in the Plymouth Laboratory in 1889, on calcareous algæ.

A. Coloured Plants—*continued.*

2. Chlorophyceæ (Green algæ).
    i. Filaments segmented.
    α. Filaments monosiphonous, conferva-like.
        a. Joints often irregular; branches separated at the base by a wall.—*Gomontia.*
        b. Joints regular, cylindrical; branches without basal wall.—*Siphonocladus.*
    β. Filaments anastomosing, producing parenchymatous expansions.—*Zygomitus.*
    ii. Filaments unsegmented.—*Ostreobium.*
3. Phycochromaceæ or Cyanophyceæ (Blue-green algæ).
    a. Nostocaceæ.
    i. Filaments very much branched, provided with hairs and lateral heterocysts.—*Mastigocoleus.*
    ii. Filaments simple or little branched; without heterocysts and hairs.
        α. Filaments very fine, 0·95 to 1·5 μ thick, branched·—*Plectonema.*
        β. Filaments simple, 4–6 μ thick, a fresh-water plant.—*Phormidium.*
    b. Chamæsiphonaceæ.
        Trichomes composed of distinct cells, contents of which finally divide into secondary cells. Plant very refractive.—*Hyella.*

B. Plants Colourless, appearing to belong to the *Fungi.*
    1. Filaments very fine, straight, uniform, unsegmented.—*Ostracoblabe.*
    2. Filaments irregular, presenting globular swellings.—*Lithopythium.*

Perforating Thallophytes (algæ and fungi) have been found in foraminifera, corals (recent and fossil), calcareous pebbles, fish-scales, and molluscan shells (empty and living), and may yet be found in calcareous rocks. They are to be found from the littoral zone to a depth of 1,095 fathoms, from the Arctic regions to the Equator, and 14° S., and at Cape Horn. They occur in sea- and in fresh-water. Some genera are extremely rare, having been seen on one occasion only. Others are very common. Thus, on the northern shore of Dublin Bay I should have no great difficulty in obtaining a thousand specimens of the razor-shell and others attacked by *Gomontia*, and, in all stages of disintegration, as a result of its boring activity. There can be no doubt that there is every probability of large additions to our knowledge of the group by painstaking inquiry, both in the completion of the life-histories of the forms already made known, and in the discovery of new species.

REFERENCES.

1. **Natural Science**, "The Growth of a Madrepore Coral," Feb., 1894, p. 85.
2. **Bornet et Flahault.**—"Sur quelques plants vivant dans le test calcaire des mollusques." *Bull. Soc. Bot. France*, t. 36, 1889, pp. 147–176, plates vi.–xii.
3. **Duncan, P. M.**—"On some Thallophytes Parasitic within Recent Madreporaria." *Proc. Roy. Soc.*, 1876, no. 174.
4. **Lagerheim.**—"Note sur le Mastigocoleus." *Notarisia*, 1886, i., p, 65, tab. 1.
5. **Batters, E. A. L.**—Conchocelis rosea (a perforating red alga). *Phycological Memoirs*, ii., 1893.

T. Johnson.

The Laboratory, Glasnevin, Co. Dublin.

II.

An Address to the Museums' Association on the Museums of Dublin.[1]

ON behalf of those who, some months ago, formed themselves into a Reception Committee in order to make arrangements for this visit of the Museums' Association to Dublin, I have much pleasure in offering you a hearty welcome. Our only cause of regret is that more of the members have not found it possible to visit us on this occasion. Indeed it has been a great disappointment to those who have laboured to make this meeting a success to have received letters of apology from several whose urgent engagements have prevented them from being present with us to-night.

For the honour you have done me by electing me as your President for this year, I beg to offer you my sincere and grateful thanks.

The addresses of several of my predecessors have dealt with the general principles which should govern the administration of Museums; on the present occasion I propose to describe what may be referred to as the evolution of a Museum which has a very wide and comprehensive range—a wider range, perhaps, than any other Museum, putting the British Museum out of the question as being beyond comparison, and scarcely excepting even the great National Museum of America at Washington; for although our affiliated Museum in Edinburgh is in some respects more extensive, it does not include within its walls an archæological collection similar to ours.

In many cities, notably in Berlin, the process of decentralisation or specialisation of Museums has been carried to a considerable extent; and, while there may be much to be said in favour of specialisation where there are extensive resources to draw upon, on the other hand where this is not the case it cannot be denied that there are certain advantages to be obtained from centralisation.

In a comprehensive Museum no one section is wholly dependent on a single officer and a small staff, while all may derive advantages from co-operation. Uniformity and continuity of method are secured,

[1] This address was delivered on the 26th June by Professor V. Ball, C.B., LL.D., F.R.S.

and all branches receive the benefits which are derivable from the workshops, skilled artisans, and plant which can alone be efficiently maintained in a large central institution.

It is a minor, but at the same time an important, matter to remember, too, that miscellaneous contributions and bequests to an institution with many branches can generally all be dealt with and provided with proper places in the various sections; but such donations to a specialised Museum are often out of place, and cannot, or at least should not, be exhibited. Moreover, there are many objects which belong to the borderlands of Science or of Art, or of some of their distinct subdivisions respectively, and for such objects, instead of having one location, it may be necessary, wherever there are distinct Museums, to have several more or less complete repetitions. It will be admitted, I think, that, generally speaking, it is undesirable that two or more Museums in the same city should overlap in their ranges by exhibiting the same or similar objects, or by competing at sales for their purchase; the advantages of that state of things being indeed restricted to the sellers, none being felt by the public at large, whether as ratepayers or visitors to the Museums.

The position of our great central National Museum here may be compared to a river into which many tributaries have poured their contents; but to prevent any misconception from the use of that term, they should rather be called contributaries. Although the sources of the main stream in this case stretch far back into a now remote past, I purpose to deal with these contributaries first, giving brief sketches of each of them, and omitting such details as it would be tedious, even if it were possible, to deal with on the present occasion.

On the subject of Provincial Museums in Ireland I shall say very little. Attached to the Queen's Colleges at Belfast, Galway, and Cork there are Museums which are primarily intended for teaching purposes, but are also open to the public. At long intervals I have seen all of them, and each, I believe I am correct in saying, represents more or less the views of the Professors respectively who have had charge of them, controlled, no doubt, to a considerable extent by the amount of funds which have been available.

There are two other Museums in Belfast, namely, that belonging to the Corporation, and that of the Natural History Society. Already these and the College Museum overlap in some departments, which is perhaps unfortunate; but that is a matter which it is not for me to comment upon further here, as I know there are difficulties connected with it, and I do not venture to suggest a remedy, though what one suggestion would be is sufficiently indicated by the remarks made above upon our centralised system in Dublin.

Minor Museums exist in Larne, Armagh, Kilkenny, and possibly in some other towns; they owe their origins to the activity of local societies or of individuals, some of whom have passed away, and although they contain objects of interest and value, they include some

others which are rather out of place, and their development is hampered by lack of funds and other causes.

Although, through the kindness of the Rev. Dr. George Stokes, I have been placed in possession of much interesting historical information regarding the Scientific Societies of Dublin in the 16th and 17th centuries, some of which appear to have possessed collections, I have not as yet had the necessary leisure for following up exhaustively the clues which he has placed in my hands, nor, indeed, could I devote in this address the time and space necessary for doing full justice to the subject; but I have seen enough to prove that the task, if undertaken, will be productive of interesting results, and that it will show that in those early times a spirit of active scientific enquiry was already in existence in Dublin, in which such men as Sir Wm. Petty, Molyneux, Marsh, Pococke, Rutty, Madden, Keogh, Berkely, St. George Ashe, Robert Boyle, and others whose fame was not merely local, actively participated. Some of their contributions to science are well-known, having been printed in the early volumes of the "Philosophical Transactions." There was, indeed, a very intimate relationship at the time between at least one of these societies and the Royal Society of London.

Trinity College.

This grand institution, which for upwards of three centuries has maintained its high position, successfully accomplished the aspirations of its founders, and secured for itself respect and admiration all the world over as a centre of light and learning, has founded within its walls, and keeps still in active operation, several Museums which, with the progress of scientific education, have been gradually more and more specialised to meet the educational necessities of the present day, as follows:—

I.—Museum of Natural Philosophy.—Founded about the year 1730. It contains a collection of various philosophical instruments.

It is in the charge of the Professor of Experimental Philosophy.

II.—Museum of Anatomy and Zoology.—The Zoological Museum was founded in the year 1777, and contains a large series of typical specimens.

The objects of most general interest in this Museum are the skeleton of the Irish giant, Magrath, and the only existing Irish example of the Great Auk.

Many of the older specimens were formerly in the private Museum of the late Dr. Robert Ball, whose collections were merged in those of the University in the year 1844, and who, up to the year 1857, was Director of the Museum. It has since been in the charge of the Professors of Anatomy and Zoology.

The Anatomical collections, formerly combined with those of Pathology, are now arranged in the galleries of this Museum.

There is a Zoological Laboratory attached, and the building is close to the School of Anatomy.

III.—Anthropometric Museum and Laboratory.—A portion of the Zoological Museum has been set apart for the above purpose.

IV.—Museum of Geology and Mineralogy.—This Museum consists chiefly of teaching collections of fossils, minerals, and rocks, but there are some fine specimens of the Irish Elk and of Reptilian fossils.

About the year 1850 the collections of the Geological Society were deposited in this Museum. Catalogues of the Mineral collection were published in 1807, 1818, and 1850. Formerly it formed only a section of the Natural History Museum, but it is now in the charge of the Professor of Geology and Mineralogy. Its first arrangement in an instructive and most attractive form, in its present habitation, in 1853, was carried out by the Rev. Dr. Haughton during the earlier years of his occupancy of the Chair of Geology.

V.—Museum of Engineering Models.—Founded in 1846. It contains a variety of Engineering Models illustrating the progress of invention in the steam engine, the construction of bridges, etc.

It is in the charge of the Professor of Engineering.

VI.—Museum of Surgical and Medical Pathology.—Is intended for students in Medicine and to illustrate the Professors' lectures. It has recently been re-arranged and placed in a more spacious building attached to the Medical School.

VII.—Herbarium and Botanic Gardens.—The Herbarium contains:—

I. Plants of Great Britain and Ireland.

II. General Herbarium,

Also a laboratory for research and demonstrations.

The Herbarium is in the charge of the Professor of Botany, Dr. E. Percival Wright, who has recently published an interesting account of it in the "Book of Trinity College."

The College Botanic Gardens (situated in the suburbs) contain about eight acres of highly-cultivated ground, including several glass-houses.

VIII.—Museum of Materia Medica.—Contains specimens of drugs; is in charge of the King's Professor of Materia Medica and Pharmacy.

The Library of Trinity College has of late years developed a section of Antiquarian exhibits of considerable interest. Here may be seen the Book of Kells, the original of the Irish Harp, the so-called harp of Brian Boroihme (Boru), and many other treasures.

In the year 1868 a distinguished representative of the University, the late Dr. Henthorn Todd, stated in evidence before the Science and Art Commission which sat in that year that, while not committing the University, it was in his opinion its duty to aid a National

Museum by contributing by donation, or loan, such collections as it could spare without trenching on the teaching collections proper.

This view of their position has been practically adopted by the Board, and the following collections have been transferred to the custody of our Museum.

1863. 150 casts of Irish fishes, prepared under the superintendence of the late Dr. Robert Ball.

In 1882, a portion of Captain Cook's collection and the Haliday collection of insects.

In 1883, a collection of fossils and casts of fossils of mammals and reptiles from the Sivalik Hills.

In 1888, a large number of bundles of duplicates of exotic plants.

[In 1894, weapons and implements of the Cook collection, to be deposited on loan.]

All the above collections are arranged, and are, or will shortly be, open to the public. They have been labelled, and of some portions there are detailed printed catalogues.

The Royal Irish Academy.

The Royal Irish Academy was incorporated by Royal Charter of George III., on the 28th January, 1786, for the purpose of advancing the study of Science, Polite Literature, and Antiquities in Ireland. From its commencement it was considered by the Irish Parliament to be a proper object for public bounty, and it has since continued to receive Government aid in carrying out its work.

It has been said that a remark by Sir Walter Scott animadverting on the fact that Ireland contained no National Collection, although abounding in Antiquarian remains, was connected with the inception of the Academy's Museum. Be that as it may, we find that on the 24th June, 1839, a real beginning was made by the presentation to the Academy of the Cross of Cong by Professor MacCullagh, and of two gold torques, found thirty years before at Tara, and presented by Dr. Petrie, on behalf of the subscribers who had purchased them for this purpose. In their Report, dated March, 1841, the Council state that they consider that the formation of a Museum is an object which the Academy should continue steadily to pursue. A sum of money was allotted to the Museum Committee, and reference is made to a subscription just then raised of upwards of £1,000 to enable the Academy to purchase the collection of antiquities of Dean Dawson, of St. Patrick's. Space and time fail me to describe the acquisition by the Academy of the Petrie and other collections which were subsequently added. Even Sir Wm. Wilde's Catalogue, published in the years 1861–2, which served to spread abroad the reputation of the Academy's Museum, was not absolutely exhaustive of its contents.

In the year 1877, after some correspondence, the Academy

accepted the proposition of the Government that it should transfer its Museum to the Dublin Museum of Science and Art as soon as the building should be ready to receive it, one of the conditions being that it should be retained in Ireland for the benefit of the public.

The actual transfer of the collection to the new building did not take place till after the rest of the Museum had been opened to the public on the 29th August, 1890, but as the cases were ready for the reception of the specimens on their arrival, they were placed on view and the Gallery opened to the public within a fortnight's time after the move was commenced by officers of the Science and Art Department specially deputed for the purpose; and on the 29th October the lists of the specimens were duly signed in witness of the transfer having been accomplished.

Since then much has been done with reference to the better mounting and arrangement of the specimens in this collection, which naturally, as being the special production of Ireland, absorbs a large portion of the attention of visitors, for whose benefit and instruction a very detailed system of labelling with printed labels has been commenced.

To this great and most important section of the Museum I commend your special attention.

The Museum of the Royal College of Surgeons.

The history of this Museum covers about the same period as that of the College itself, *i.e.*, it was founded in 1784, and in 1795 the first catalogue was prepared. In 1820 it was decided to form a Natural History Museum, in addition to the collection of Anatomical preparations. As a very full account of this Museum exists in the "History of the Royal College of Surgeons," by Sir Charles Cameron, it is not necessary to give further details here. To our Museum it has contributed some specimens; among them may be mentioned a collection of fossil bones, and casts of fossils of Sivalik Mammalia.

But the most important contribution was its Curator, the late Dr. Alexander Carte, who, in 1851, was transferred to the Royal Dublin Society as Director of their Museum, and with it he subsequently passed into the service of the Science and Art Department in 1877.

Geological Society of Dublin.

The first annual meeting of the Society was held on the 8th February, 1832, an address being given on the occasion by the Rev. Bartholomew Lloyd, D.D., F.T.C.D., President of the Society.

During the following quarter of a century or so, the Society acquired a collection of rocks, fossils, and minerals, which, about the year 1850, was deposited in the Museum of Trinity College, where for many years the meetings of the Society were held.

In the year 1864 the Society's title was enlarged by a "Queen's Letter" to that of The Royal Geological Society of Ireland. For the

last few years its operations have been in abeyance, no meetings having been held.

The Royal Zoological Society of Ireland.

This Society, founded in the year 1832, though not ostensibly connected with the formation of a museum, has in two capacities contributed very materially to the acquisition of specimens, besides which it may be claimed for it that it has in a very important degree served to maintain a vigorous interest in practical zoology in our midst. Here first, for instance, was the practicability demonstrated of exhibiting living pictures to the public of the subaqueous existences of the lower orders of animal life. In spite of subsequent claims which have been made, it is known to those competent to speak with authority that the late Dr. Robert Ball invented, and so far back as 1854 put in practice at the Zoological Gardens, the system of Aquaria which has since received such a wonderful development throughout the world. The original building in which the tanks were let into the wall and were aërated by an ingenious device, still exists in the gardens, though now used for another purpose. Of the two ways in which the Zoological Society has contributed to the formation of Museums the more obvious is, of course, in the supply of specimens of animals which have died in the gardens. Of them a goodly number have found their way either to the Museum of Trinity College, that of the College of Surgeons, that of the Dublin Society, or its successor the Science and Art Museum. Not only have specimens for stuffing been thus provided, but anatomical subjects, including anthropoid apes, the larger carnivora and elephants, have, in the hands of Haughton, Macalister, Cunningham, and others, afforded most important results. As the recipient of donations and bequests of collections and valuable specimens, which have been transferred to our Museum, the Zoological Society also deserves to be mentioned. And in this connection the magnificent and unique specimen of *Plesiosaurus cramptoni*, presented by the late Sir Philip Crampton, and a fine collection of Irish birds, bequeathed by the late Thomas W. Warren, may be referred to.

Natural History Society of Dublin.

This Society was founded in 1838 for the sole object of elucidating the natural history of Ireland. It was proposed to effect this by making a standard collection of specimens and by holding meetings. The collections grew rapidly, and they were for a time open to the public ; but financial pressure rendered it impossible to continue this and, at the same time, meet the cost of publishing the papers read at the meetings.

In the *Proceedings* of the Society for November 13, 1857, and *Nat. Hist. Rev.*, vol. v., 1858, there will be found an enumeration of the specimens contained in the Museum.

By the close of the year 1869 the Society was obliged, by want

of space, to adopt measures for the disposal of its collections and library, and subsequently, about the year 1880, some of the rarer stuffed birds were transferred to the Dublin Society's Museum. Since then the Society has suspended operations. But the Naturalists' Field Club is now a flourishing young society.

Geological Survey.

The Geological Survey commenced existence as a branch of the Ordnance Survey in 1832. It was placed under the charge of Captain Portlock. Large collections were made of zoological and botanical, as well as of geological, specimens. In 1844 the Government of the day detached the Geological from the Ordnance Survey, and created it into a separate branch, Sir H. T. De la Beche being appointed Director-General.

As provided for in Lord Sandon's letter of the 9th February, 1876, the collection of the Geological Survey, consisting mainly of specimens of the rocks and fossils of Ireland, together with a few British and foreign examples for comparison, was transferred from the College of Science during the year 1890, to the gallery in our Museum specially prepared for its reception. On being thus for the first time fairly spread out for examination, it became apparent that there were not a few gaps, but these have been to a large extent since supplied by the labours of the officers of the Survey, in whose custody the collection still remains. And a very interesting and instructive collection you will find it now to be.

Museum of Irish Industry.

It was decided by her Majesty's Government in 1845—Sir R. Peel being Prime Minister—to establish in Dublin an institution somewhat on the plan of the Museum of Practical Geology in London, but more extended in its objects, as it was to embrace the general range of the Industrial Arts. Lord Castlemaine's house in Stephen's Green was taken for the purpose. The preparations of the galleries lasted from 1847–1852.

In addition to the more purely Exhibitional Department, situated in the galleries, there were at first established as portions of the institution:—

1. A Government School of Science applied to Mining and the Arts.

2. A Chemical Department, with laboratories, first termed "Museum of Economic Geology," afterwards changed to Museum of Industry. In 1853, it was transferred from the Department of Works to that of Science and Art, then newly created as a branch of the Board of Trade.

This Museum was abolished as such in the year 1865, and certain of the collections have since been transferred to our National Museum collection.

The Science and Art Museum.

The early history of this Museum, for nearly a century and a half previous to the year 1877, when it was transferred to the Science and Art Department, forms a part of the general history of the Royal Dublin Society, and I propose, therefore, in the first place, to give a sketch of the history of the Society, with special reference, however, to the inception, growth, and development of the Museum, which constituted but one department of the Society's many-sided functions.

The Society had its origin in a private association, formed in the year 1731 by fourteen citizens of Dublin, for improving husbandry, manufactures, and other useful arts and sciences. This association was incorporated by charter of George II. in 1749, and was granted a yearly sum of £500 out of the privy purse.

On the 22nd of February, 1733, the Museum was first opened to the public. Its contents then consisted chiefly of agricultural implements and models of flax and cider mills, which, with the permission of the Lords Justices, were exhibited in a vault under the House of Parliament. The first eight meetings were held in Trinity College, in the rooms of the Philosophical Society. Afterwards one of the committee rooms of the House of Parliament was used for the same purpose.

The first regular place of meeting by the Society was acquired in 1756 in Shaw's Court, from whence it removed in October, 1767, to Grafton Street, and thence again in 1796 to Hawkins Street, where a specially built edifice afforded accommodation to the Museum, laboratories, etc.

In 1761 the Society received its first Parliamentary grant, and since that time many special, besides annual, grants have been made, which were entrusted to the administration of the Society for the benefit of the public. During the latter years of the last and the early part of the present centuries the Mineral collection received many acquisitions both by purchase and donation, and for a long time it formed the most important part of the Museum.

In 1790 Parliament directed (30 Geo. III., c. 28) that a Botanic Garden should be formed, and in 1792 a cabinet of Mineralogy was sanctioned, and in the following year a sum of money was granted (33 Geo. III., c. 13) for erecting a suitable repository for it.

In 1800 the Society received a grant from the Irish Parliament of £1,500, and for twenty years after the Union the income amounted to £10,000 (Irish currency) per annum. The resources of the Society were so far husbanded that in December, 1814, it was enabled to purchase Leinster House from the Duke of Leinster for £20,000, and in the following year the collections were transferred there. By the removal of the National Library to the new buildings in 1890, the rooms of that splendid mansion have been freed from the cases which concealed so much of their noble proportions and artistic decorations. The rooms have been restored of late years, and now present an

appearance the beauty of which you will have an opportunity of seeing during the next few days.

In 1819 the grant to the Society was reduced to £7,000.

In 1820 the Society assumed the title "Royal" on the occasion of George IV. becoming the patron, and in 1832 the Lords of the Treasury directed a reduction in the Society's estimates to the amount of £1,600, and that the apartments and collections should be rendered accessible to subscribers, who need not be elected members of the Society.

In the year 1836 a Select Committee of the House of Commons reported in favour of an extension of Leinster House, in order to afford increased museum accommodation and recommended an expenditure of £3,000 for this purpose. It was, moreover, affirmed to be expedient, in reference to future grants of public money, that it should be fully understood that the members of the Society must be regarded as trustees administering public money, and that a clear and distinct guarantee should be given by the Society that the public were entitled to the full and entire use of the property. In response to a special request by the Treasury, this view of the position of the Society was admitted by a resolution passed on the 22nd of December, 1836, and on the 11th of May, 1837, a further resolution of the Society affirmed that, in consideration of the Parliamentary grants made to the Society, it was just and reasonable that it should not alienate any of its property without the approval of the Lords of the Treasury.[2]

In the year 1840 the Irish Government entered into correspondence with the Society, pointing out that it had not carried out certain recommendations with regard to its development as a scientific institution, to which the Society replied that it was not, nor was it ever intended to be, a scientific body; but in this respect its intentions have since undergone considerable change. In 1841 the Parliamentary grant was withheld, but was restored in the following year. In the same year the Society established an Agricultural Museum; in it the principal objects were implements, seeds, models of fruits, vegetables, and veterinary preparations. In 1844 a sum of £150 was voted for the annual support of this Museum.

In 1842 a special extra sum of £300 was voted by Government to the Society for the purpose of providing gratuitous public lectures in provincial towns—a system which had been first established in 1839. The regular annual grant to the Society at this time was £5,300.

In 1843 the Society memorialised the Government for a special grant of £3,000 for the formation of a National Museum, and £4,000 for renewal of buildings in the Botanic Gardens. In this memorial it was pointed out that while the annual grant had diminished from £10,000 in 1800 to £5,300 for the current year, that to the British Museum had increased from £3,000 in 1804 to £81,000.

[2] According to a Treasury Report, issued in 1876, the public grants made to the Society between 1801 and 1835 inclusive amounted to £293,705.

Already part of that sum of £3,000 had been expended by the Society on the building for the new Museum, but the grant which was applied for was refused by the Lords of the Treasury, and the building when completed was never used as a Museum, having, in the year 1849, on the recommendation of Mr. Poynter, been handed over to the new School of Design under the Board of Trade. As it had been built by the Society, a claim for a compensatory grant was put forward, and in 1853 the Government voted a sum of £5,000, provided half that amount was raised by public subscription, for building a Natural History Museum. It was proposed to make it a truly National Museum, and an efficient adjunct to the "educational functions of the Society, and especially to the Schools of Art." The foundation of the Museum was not laid till three years later, namely, on the 7th of April, 1856, the ceremony being presided over by Lord Carlisle, who was then Lord Lieutenant. As had been fully provided by Act of Parliament (17 & 18 Vict. c. 99), its design was in harmony with that of the intended National Gallery, which was also to contain Marsh's Library.

Dargan's Great Exhibition was held on the Society's grounds in 1853, but it is no part of this account to deal with it, nor with any of the other exhibitions and shows which have been held under the Society's auspices.

In 1854 the Society first commenced to report its operations to, and was placed under the Science and Art Department of, the Board of Trade.

At about this period the Museum of Irish Industry had developed somewhat beyond its originally intended scope as a technical Museum; this led to some controversy with the Royal Dublin Society, which was intensified by the fact that the professors had been transferred from the Society to the first-named institution.

In 1857 the completion of the building of the new Natural History Museum, on the occasion of the visit of the British Association to Dublin, was inaugurated by the delivery of lectures by Sir Wm. Thompson on the Atlantic Telegraph Cable, and by Dr. Livingstone on his African discoveries.

In 1858 an Exhibition of Decorative Art was held in the new Museum, to which a loan collection was contributed by the South Kensington Museum.

In 1859–60 the Natural History Museum received important loans from the Royal Irish Academy and from the Royal Zoological Society. A well-known collection of shells, by bequest from Thomas W. Warren; donations of birds and insects from British Columbia, by Dr. (afterwards Sir) Charles Lyell; and a highly valuable collection of Arctic mammals, birds, fossils, and minerals from Captain (afterwards Admiral Sir) Leopold M'Clintock, who continued for many years to be a contributor to the collections, were also received.

Much pressure was at this time used by the Government, even

to the extent of threatening to withdraw the annual grant of £6,000, in order to induce the Society to consent to open the Botanic Gardens at Glasnevin on Sundays. At last the Society consented, and on Sunday the 18th August, 1861, the first opening took place, and the total attendance for the first twelve Sundays amounted to 78,132.

In 1862 a report on the Society was drawn up by a special committee, in which its position and its past relations with several departments of the Government were discussed, and the question as to whether it was a private body or a public institution, chartered for the administration of public funds, was specially dealt with. In this same year a Treasury Commission was appointed to inquire into the Society and other analogous institutions in Dublin, which were supported wholly, or in part, by public grants, and to report thereon.

During the year 1863 the enlargement of the powers of the Council by charter, and a proposal for amalgamation with the Museum of Irish Industry, which had been founded in the year 1845, were discussed.

In consequence of the lack of funds for additional case accommodation, very slight progress was made during the year in the permanent arrangement of the Museum. The Society presented a special memorial for the grant of a sum of money for perfecting the several institutions as follows :—

Museum of Natural History	£4,500
Library	1,000
Botanical Museum and Garden, Glasnevin	1,000
Agricultural Department	3,000
Total	£9,500

This sum, it was understood, had been considered requisite by the Treasury Commission, which had reported as above in 1862. The delay in its recommendation to Parliament arose from the fact that the question of the future position of the Museum of Irish Industry was still under consideration.

In 1864 a Parliamentary Select Committee assembled, on the 20th April, to enquire into the Scientific Institutions of Dublin which received aid from the State. One object of this enquiry was to obtain a revision of the Treasury Report of 1862, and of certain recommendations therein specified, the most prominent of which were :—

I. The amalgamation of the Museum of Irish Industry with the Royal Dublin Society, and the reduction of the staff of lecturers and teachers.

II. The amalgamation of the pecuniary grant to the Royal Dublin Society.

In 1867, Dr. Alexander Carte being then the Director of the Natural History Museum, the staff was increased by the appointment of two

Assistants, Messrs. A. G. More and W. F. Kirby, both of whom, as is well known—especially the former—did much good work for the Museum. Mr. Kirby subsequently received an appointment in the British Museum.

Towards the end of 1867 considerable progress had at last been made in the arrangement of the collections, and numerous valuable donations were received. In the following year, 1868, the first steps were taken towards opening the Museum in the evenings—one evening, Wednesday, being selected as an experiment—otherwise the Museum was only open for four days in the week.

It may be of interest to record here, in connection with the project of establishing a Department of Science and Art in Ireland, that "A Memorial from 245 Students of the School of Art, was addressed to the Lords of the Treasury, praying that, in the event of a Science and Art Department being established in Ireland, the present right of Irish Students to compete with those of England should be preserved."

In September the Council was informed that a Commission had been appointed "to consider the establishment of a Science and Art Department for Ireland." The report of this Commission contains much interesting information. It decided against the formation of a local Department, and sketched out a plan for the establishment of a State Museum.

In 1869 the question of opening the Museum on Sundays was under consideration, but action was deferred, and in fact the proposition was not carried out till the year 1884, or 15 years afterwards.

In 1870 the Department of Science and Art, pending negotiations which were in progress, declined to recommend Parliament to vote a sum for the erection of a Museum of Ornamental Art.

The acquisitions of the Museum in this year were again very considerable.

It was resolved to open the Museum on two evenings in the week (Mondays and Thursdays) and to close it altogether on Tuesdays and Fridays. Afterwards these days were altered.

The evening attendances far exceeded those in the day, having been 89,338 and 13,899 respectively, or an annual total of 103,237. On one evening upwards of 3,000 persons attended, and some confusion was caused by the consequent overcrowding.

From a report by the Committee of Natural History, it would seem that there was much dissatisfaction with the Natural History building. It was stated to be in several respects unfitted for the use to which it was applied, and was not on a scale worthy of being called national. The Committee also claimed that the officers should be put on the same footing, as regards salaries, as the corresponding officers in the British Museum.

In 1872 the *Proceedings* of the Society contained a report by

a committee appointed to consider the question of a General Museum of Science and Art in connection with the Society. It concurred in the opinion of the Commission which met in Dublin, 1868, "that the formation of a separate Department for Ireland would be detrimental to the interests of Science and Art in that country." It then refers to the proposal of the Commission that the several proposed institutions should be concentrated on one site, namely, a Science and Art Museum, a Public Library, a Museum of Natural History, a Museum of Irish Antiquities, a National Gallery and a School of Art.

Plans were prepared by which it was shown how this object might be attained on the Society's premises by the incorporation of Leinster House, and the connection of its first floor as a means of communication between the proposed buildings. The committee demurred, however, to the control of these institutions being transferred to another authority.

The Lord Lieutenant, Earl Spencer, to whom the report was forwarded, expressed himself strongly in favour of the establishment of a "General Museum of Science and Art," but considered that the plan would not be perfect without the co-operation of other societies.

The most important donation to the Museum during the year was made by Sir Richard Griffith. It consisted of his unique collection of Irish fossils (Carboniferous and Silurian), which contains nearly all the typical examples described by M'Coy and Salter in their "Synopses of the Carboniferous Limestone and Silurian Fossils of Ireland," respectively. It serves to illustrate the state of palæontological science at the time of the publication of Sir Richard Griffith's large map. It is now well placed for inspection in the Palæontological Gallery.

In a report by the Council of the Society, read on the 5th of March, 1874, the delay in providing funds to meet several urgent applications, and in the provision of the new Museum, etc., is animadverted upon.

A committee appointed to report on the question of the extension of the buildings for the Museum, Library, etc., confirmed, except in some minor details, the report of the committee of 1872.

The premises of the Society were visited and inspected on the 7th of December, 1875, by the Right Hon. Viscount Sandon, M.P., Vice-President of the Committee of Council on Education, Mr. W. H. Smith, M.P., Secretary to the Treasury, the Right Hon. Sir Michael Hicks-Beach, M.P., Chief Secretary for Ireland, and Major (now Major-General Sir John) Donnelly, R.E., with reference to the increased accommodation for the Museum and Library, and the formation of a general Museum of Science and Art, and a letter was received by the Society on the 17th of February, 1876, from Lord Sandon, relative to the establishment on the Society's premises of a general Museum of Science and Art. An objection was at this time raised by the agent of the

Pembroke estate to the proposition, then current, to erect the buildings for the new Museum on Leinster Lawn. Various deputations from the Society to the Lords of the Committee of Council on Education, for the purpose of discussing the terms of Lord Sandon's letter, took place at this time.

A proposition which was made, for the amalgamation of the Royal Irish Academy with the Royal Dublin Society was put forward during the year, in the hope that it would facilitate the settlement of the pending question as to the new Museum, but it did not meet with acceptance.

The *Proceedings* contain several references to the bequest by the late Mr. J. H. Foley, R.A., the eminent sculptor, of his models of statues, busts, etc., to the Royal Dublin Society.

On November the 18th a letter was received from Mr. W. H. Smith, Secretary of the Treasury, stating that it was intended to obtain an Act of Parliament "to vest all the buildings and land now held by the Royal Dublin Society in the Government" in order "to afford the State the requisite control over property and buildings upon which a large amount of public money is to be expended."

A resolution was thereupon passed by the Society to the effect that it was desirable to facilitate the acquisition by Government of the sites necessary for the several institutions, provided that "the rights of existing members be preserved undiminished."

The volume of *Proceedings* of the early part of the year 1877 contains frequent references to the negotiations which were carried on between the Society and the Government in reference to the establishment of the Science and Art Museum.

An agreement supplemental to the provisions of Lord Sandon's letter was entered into between the Government and the delegates appointed by the Society, by which several important concessions were made in consideration of the property and rights hitherto possessed by the Society, but which were now relinquished; and accommodation for the Society in Leinster House was secured to it on the same terms as govern the occupation in Burlington House by learned societies.

By a letter of the Science and Art Department, dated 9th August, Dr. Steele was appointed to carry on the administration of the Natural History Museum, School of Art, Library and Botanic Gardens, as soon as the Bill should receive the Royal Assent. He was to have the same authority of administration as had been exercised by the Council of the Royal Dublin Society, except as regards the appointment of superior officers; and the funds for carrying on the work were to be those provided in the current estimates.

The total of the grants which had been entrusted to the Society for administration on behalf of the public, inclusive of Public Works vote, amounted, from 1801 to 1877, to £627,056; the regular income

of the Society from members' subscriptions for the same period having been about a tenth of that sum, and the subscriptions for special purposes having yielded about £4,000.

Having thus brought down the history to the year 1877, when the complete transfer to the Government of the buildings and collections was concluded by the payment to the Society of a total sum of £35,000, including compensation for disturbance of the cattle shows, which formerly used to be held on the premises, I now proceed, I fear somewhat imperfectly, because of necessity briefly, to describe what the main features of the collections were which were so transferred.

These consisted mainly of zoological specimens in the Natural History Museum which had been acquired during a long course of years by donation and purchase. There was also an extensive Mineral collection, though not up to date. There were numerous isolated Geological and Palæontological collections, for the exhibition of which but limited space had been available; these included Sir R. Griffith's collections, and several valuable collections from the Arctic regions. There was a nucleus of an Ethnographical collection, a few antiquities, and some other miscellaneous objects, besides a collection of agricultural implements, veterinary specimens, and seeds, and a Botanical collection and Herbarium in the Botanic Gardens. The Art collections consisted mainly of casts and water-colours in the School of Art, and there was the then unsecured reversion to the Foley Collection of Casts.

The First Annual Report of the first Director of the Science and Art Museum, Dr. William Edward Steele, who for 25 years previously had been Assistant Secretary and Registrar of the Royal Dublin Society, was dated January, 1879, and it covered the period from the 14th August, 1877, when the Dublin Science and Art Museum Act passed, up to the close of 1878. It describes the initial steps taken in the formation of the Art Department of the Museum, which has since then grown to such important dimensions that it almost fills the large building completed for its reception less than five years ago. A special loan collection of scientific and art objects was prepared for exhibition in the old Museum on the occasion of the visit of the British Association in 1878.

In that year a local Board of Visitors of the Museum and Botanic Gardens was appointed, in accordance with the terms of Lord Sandon's letter above referred to.

During the year 1879 the want of space for exhibiting the growing art collections was keenly felt. Mr. Longfield was appointed to assist the Director in this department of the Museum. Among the acquisitions were the Foley casts, to which reference has already been made. For the Natural History Department the unique fossil of *Plesiosaurus cramptoni*, which had been lent to the Museum in the year 1861, was purchased from the Zoological Society. There were also many other purchases and presentations.

In 1880 the question of the site for the new Museum was under discussion, and the opposition to the proposed site on Leinster Lawn, principally by certain inhabitants of Merrion Square, led to its abandonment in favour of the other (west) side of Leinster House.

In this year the Ethnographical collection, to which some important additions had then recently been made, was taken in charge by the Art Department of the Museum, and was re-arranged and labelled. Already it formed a nucleus around which the considerable acquisitions of the last fourteen years have since been added, and it now constitutes a very important section in the new Museum building.

In the year 1881 a number of architects submitted designs for the new Museum, and some of them were selected to make detailed plans for the final competition.

In this year Dr. Alexander Carte died. For thirty years he had controlled the Natural History Museum. Among other original devices of his for the better exhibition of specimens was the beautiful arrangement of the birds, which is still in operation as he left it. It possesses manifold advantages over the shelf system, adopted in most Museums. He was succeeded by Mr. A. G. More, his senior assistant.

In 1882 unavoidable delays arose to postpone the first steps being taken towards the erection of the new Museum; but by the close of the year the temporary gallery for the reception of the Art collections was nearly completed, and it was opened to the public about six months later.

Among other acquisitions which were received was an important addition to the Ethnographical collection, consisting of a portion of Captain Cook's original collection of South Sea Island weapons, dresses, etc., presented by Trinity College. A valuable donation of Sèvres China was also received from the French Government. In the Natural History Department the invertebrate collections were re-arranged, and their educational value increased by the addition of glass (Blatschka) models, geological charts, etc.

In May, 1883, the Director of the Museum, Dr. William Edward Steele, died, having devoted nearly six years of anxious and untiring service to the foundation of the Art Department of the Museum, and to the many intricate questions connected with the transfer from the Society to the Science and Art Department, of the Museum and the other institutions with which the office of the Director is connected.

In September of the same year, being at the time Professor of Geology and Mineralogy in the University of Dublin, I was appointed to succeed the late Director, and here, perhaps, I may be permitted to refer to a fragment of personal biography. For reasons I need not enter into, the position of Director of the Museum, with the numerous extra responsibilities connected with three other institutions, namely,

the School of Art, the National Library, and the Botanic Gardens, was well known to be one of some difficulty and responsibility, and my reason for accepting it, and relinquishing a post of much greater ease and freedom of action, was mainly this, that the management of a Museum of wide range had been, I may say, the dream of my life, and the occasions when I had temporary charge of Museums in India, including that of the Imperial Museum in Calcutta, which had been just then transferred from the Asiatic Society of Bengal to the Government, under circumstances similar to those I have described above, only whetted my long-cherished desire for a permanent appointment of the kind.

In what I have to say regarding the further stages in the evolution of this Museum during the past eleven years, I shall pass without mention the difficulties which have had to be met, and the controversies they have given rise to; much as they have added to our labours and anxieties, they are not matters suitable for introduction here, but the curious may find them, or some of them, recorded elsewhere.

One of the first steps which I found to be necessary was the extension of the already established workshops in connection with the Museum; and soon after a printing press was provided, by which printed labels could be speedily prepared on the spot, and the educational value and, so to speak, the self-explanatory character of the collections extended.

Shortly afterwards application was made to the Lords of the Committee of Council for permission to open the Museum on Sundays. This was immediately granted; and the proposition, it may be added, met with no opposition whatever from any quarter, but, on the contrary, was hailed with delight by the public, who have since availed themselves of the privilege in ever increasing numbers. This was in 1884, or just ten years ago, and as in the case of the three other institutions in Dublin, which were first opened on Sundays many years earlier, with two of which I am also officially connected, the movement has been an unqualified success, and a boon which is much appreciated by the public. I very well remember the terms of approbation in which it was spoken of by the visitors when they were interrogated on the first few Sundays after the first experimental opening. Among them were working men who had never been inside the walls of the Museum before, and others who voluntarily explained that their usual occupations on Sundays consisted in driving about on cars, with an occasional visit to a public-house. It may be asked by some, How about the employment of labour on Sunday? To it I can reply that no compulsion whatever has been used. The rate of Sunday pay being liberal, we find plenty of volunteers from among the staff to supply frequent changes of a sufficient number to take Sunday duty in turn. These facts have been placed before the Society in London which has the

opening of Museums and similar institutions on Sundays in view, and my sole reason for not having taken any more active part in the operations of that Society than I have done is, that my hands are already sufficiently full with my own work here. I observe, however, that the procedure of that Society appears to have made less headway against opposition than might have been expected from the time it has been in existence.

If Dublin is, as I believe it to be, fairly free from rowdyism and disorder on Sundays, I think it may be to some extent due to the large numbers of persons who are tempted to take their families to the various resorts in and near the city, where they can enjoy so much innocent pleasure and instruction as the institutions referred to can afford.

In 1884 further temporary accommodation was provided, by which the overcrowding in both sections of the Museum was relieved, and space for new acquisitions was provided. The printing of regular quarterly acquisition lists in such a form as to be capable of being cut up into labels was commenced, and has been continued ever since. This was in addition to the descriptive labels printed in the Museum press, to which reference has just been made. A slight increase to the staff of the Art Department, by the appointment of a Technical Assistant, was sanctioned, and the employment of experts to catalogue special groups though not initiated was advanced a stage, so as to become a very important accession to the powers of the limited staff of the Museum. In this way we have since had the advantage of the services of Mr. R. Lydekker on two occasions, the late Mr. William Davis, Mr. Kidston, and Mr. Foord, who have respectively prepared catalogues of and arranged portions of the Palæontological collections; while Mr. W. F. Wakeman and Mr. George Coffey have dealt similarly with the antiquities and coins; Dr. Will, of Erlangen, with the Coleoptera, and Mr. Greenwood Pim with the Lichens. Besides this, there have been volunteer experts who have arranged minor groups.

Of the advantages to be derived by Museum officers from visiting Museums in other countries than their own, I can speak from the experiences of my colleagues and myself. I hardly ever visit a Museum, no matter how small it may be, that I do not see some object of interest, or some method of exposition that is suggestive, either of imitation, or, perhaps, even of avoidance, for it is almost equally important in Museum management, as indeed it is in other affairs of life, to know what *not* to do, as to learn what it is advisable to have done!

Referring to my own visits to Museums, I derived, I think, more direct and immediate benefit from those of America and Canada, in the year 1884, than from those of the continent, including Berlin. There is an originality of conception and method, and a breadth in execution which render the Museums of Washington,

New York, and Cambridge (Boston), particularly instructive for one who had to reorganise an old, and initiate an orderly system in a new Museum. In many of the minor Museums which I visited in the States and Canada, I also acquired suggestive ideas, which have, in due course, produced satisfactory results. [3]

In the year 1885, the principal events here were connected with the completion of the plans for the New Museum and Library, and the laying of the foundation by His Royal Highness the Prince of Wales, on the 10th of April, the ceremonial connected with which having been witnessed by about 8,000 persons.

In the latter part of the same year, the lower hall of the Natural History Museum, which had previously been the receptacle of many miscellaneous objects, was reorganised, scattered collections of reptiles, fish, and fossils being brought together and systematically arranged in suitable cases; but this work was not wholly completed till the year 1890, when the fossils were removed to the gallery vacated by the Art Collection. During the same year, Mr. A. G. More's list of Irish Birds and several other publications referring to the collections were issued, and arrangements were concluded with the Science and Art Department for the publication of Miss Stokes's "Arts of Christian Ireland."

In 1886, the work of concentrating the collections in the sections assigned to them was vigorously pressed on. The general reference series of birds' skins, belonging to about 5,000 species, was provided with glass topped drawers, and a cast of the skeleton of the Megatherium was successfully mounted. An arrangement was also made by which the majority of the specimens obtained in a dredging expedition off the West Coast of Ireland, organised by the Rev. William S. Green, were to become the property of the Museum.

In 1887, the New Museum Building had acquired sufficient form to admit of a scheme of allotment of its twenty-four rooms and two courts with their galleries. This scheme, with but slight modification, has been carried out in the arrangement of the collections; needless to add it facilitated the transfer, as each object, when leaving its old quarters, had a definite destination, and so the rooms were filled concurrently, and we were able to open to the public much sooner than would otherwise have been the case.

The scheme of allotment, with lists of the principal contents of each section, will be found in the General Guide to the Museum, to which reference can easily be made, so that the details need not be given here.

A very important alteration in the casing of the Mammal collection in the Natural History Department, by which the specimens were brought forth from the darkness of cases under the

[3] See Appendix to thirty-second Report of the Science and Art Department, 1885.

gallery into the light of their present position, was commenced, but took some time to accomplish.

Preparations were also set on foot for the exhibition of Groups, showing the breeding habits of birds, and for providing maps to illustrate the geographical ranges of individual species.

Mr. A. G. More, Curator of the Natural History Department, was compelled to retire owing to ill-health, and Dr. Scharff, the present Keeper, was appointed to succeed him.

In 1888, many very important purchases were made, and the assistance of experts in the arrangement of the collections was largely availed of.

On the 4th of November, 1889, the completed New Building was handed over by the contractors, and the work of transfer of the collections was immediately commenced. Considerable purchases were made at the Paris Exhibition.

The year 1890 was memorable, from the fact that, on the 29th of August, the New Museum was opened to the public by the Lord Lieutenant, Lord Zetland, the occasion being celebrated by a suitable ceremonial, at which a large number of persons were present. In spite of the extra work thrown on the staff, a considerable amount of other work, such as the preparation of a General Guide Book and several special handbooks, was accomplished.

Later on in the year, after the official opening, the collection of the Royal Irish Academy was transferred, as has been related on a previous page.

In the Natural History Department, the arrangement of the special collections to illustrate geographical distribution and evolution was commenced. These now constitute a notable feature in the Museum.

The Electric Installation, commenced during the year 1890, and completed in 1891, has proved a very great success. It has not as yet been extended to the Natural History Museum, where we are most anxious to be freed from the deleterious effects on the specimens caused by the gas.

The evening opening of the Museum, though the attendances are fairly good in regard to numbers, is, perhaps, from some points of view, not so satisfactory as the Sunday opening. It is, indeed, true of both that many of the visitors are less intent upon seeing the collections than they are on meeting their friends, and the crowds of people who use the place as a promenade interfere somewhat with the comfort of those who desire to view the exhibits; but a decided improvement in the quality and conduct of the evening visitors was secured some years ago, by altering the hour of closing from ten o'clock to nine o'clock, and it is difficult to devise a plan by which mere promenaders could be excluded from an institution which is open to all respectably-clad persons. The police and attendants

cannot well do more than restrain openly unseemly conduct should it ever take place.

And here it may be stated that this Museum differs from most others in the fact that it is open free to the public for every day in the year save two, namely, Christmas Day and Good Friday, the necessary cleansing being accomplished daily before the opening hour, or after the closing hour, by the regular staff; and I think it will be admitted that the Museum presents no signs of neglect in this respect.

With our own skilled artisan staff we succeeded in suspending the skeleton of a large whale, weighing about six tons, under the roof of the Natural History Museum, where it is well seen, and no longer encroaches on valuable space, as it did when supported on pillars in the basement hall. The method of suspension can be freely commended for adoption in other Museums. Your attention may also be directed to the methods of shelving devised for the Mammals and for the Palæontological Gallery; they have many manifest advantages respectively.

During the same year we received on deposit, which was afterwards changed into a permanent donation, the life-size model by the late Mr. Birch for a proposed bronze statue of Lieutenant Hamilton. The incident which this statue records, namely, the attack on the Residency at Kabul, in the year 1879, is vigorously represented by the figure of that gallant young Irishman; and the statue has been, I may add, since its erection, an object of special attraction to many visitors.

In the year 1891, some long-deferred additions to the strength of the staff were at last sanctioned; these would have been still more acceptable had they been made in response to our earnest appeals before the extra pressure of the previous two years came upon us.

Under the temporary custody of Professor Johnson, which was afterwards changed in 1892 to a permanent charge, the Herbarium and Botanical Museum advanced considerably, and, indeed, in all Departments great progress was made.

The total attendances for the three years were as follows:—1889, 218,880; 1890, 313,564; 1891, 344,071; and the Sunday attendances were 1889, 40,599; 1890, 61,386; and 1891, 58,000.

In the years 1892, 1893, and 1894, the work of arrangement has also progressed in all branches, but there are no very special circumstances to be recorded here. Additions as they arrived have, so to speak, dropped into the sections provided for them, and the various arrangements for the good order and smooth working of the Establishment have operated, I believe, with satisfaction to all those who are immediately concerned. The public attendances in 1892, 343,714, were very close to those of 1891; and in 1893 they reached their maximum, namely, 367,645, or an average of over 1,000 a day, although the Sunday attendances fell from 57,536, in 1892, to 51,894,

in 1893. During the current year the total attendances promise to be very much larger.

In preparing the lines upon which this Museum should be arranged, both with regard to its then present condition and future acquisitions, we encountered from one quarter some opposition to the idea of our having any legitimate aspirations beyond those of making it a teaching Museum, which would exhibit only such types as were requisite for instruction in the various branches with which it dealt. But it appeared to us that our proper duty under the circumstances, while by no means losing sight of the educational aspect, was to steadfastly keep in view the desirability of making it a repository, in the fullest sense of the term, of all such objects as might fitly find a place in a national institution; and that, not merely in the sense of representing the productions and industries of this country of ours, but in the wider sense of providing for the nation a means of seeing and studying the productions of the world at large, and the arts of many nations, these, as examples for imitation, to be the best of their kind attainable with the means at our disposal.

While we endeavour to provide, both for our own fellow countrymen and for visitors, fitting exhibits in all branches of Irish decorative-art, antiquities, and science, we are also glad to say that our efforts to provide exhibits from other countries have often met with warmly-expressed approval, and we rejoice now at the prospect of their being examined by the experts whom this Association has brought together.

There is one Section which is not and cannot be largely dealt with in such a Museum, namely, that of purely industrial objects. For these, we are convinced, periodical exhibitions afford the proper medium for making them known. In such exhibitions, the exhibits can, and do, keep pace with the times, whereas in a Museum they must of necessity be in some departments far behind the progress of invention, as frequent renewals up to date would be impracticable; while in other departments it would be impossible, even with enormous financial resources, to exhibit at all. How, for instance, not to speak of minor industries, could we exhibit steam or electric plant so as to be constantly up to date?

Anxious that this National Museum might be made to spread its influence over a wider area than the metropolis alone, I sometimes dream of projects by which peripatetic loan collections might be sent out from it to provincial centres in Ireland, but so far the project is in a visionary stage without concrete form. But if it is the case that as yet collections cannot be sent out to be seen by the people throughout the land, it is a gratification to find among our visitors so many tourists, not only from various parts of Ireland, but also from Scotland, and these with the large contingents which now arrive every year from the western counties of England by way of the Isle of Man, swell our numbers of average daily admissions.

Under the existing system of administration four institutions, namely, the Museum, National Library, Botanic Gardens, and School of Art, whose functions necessarily overlap in some important respects, are linked together, and so far co-operate for mutual advantage. The books in the Library are available to those working the other three institutions, and the specimens in the Museum and Botanic Gardens are available to the students of the School of Art, while the official administrative staff is common to all.

The annual Parliamentary vote for the four institutions amounts to about £20,000, besides the vote taken by the Board of Works for maintenance. This expenditure may be regarded as a moderate outlay upon the part of the State, considering that an average of 2,600 persons visit these institutions daily, when all are open together.

I have thus conducted you through, or rather past, the controversies and labours which have resulted in the present condition of affairs; and I venture to hope that your inspection of the Museum and the other institutions during the next few days will prove a sufficient reward for your trouble in coming here, and that some of you, at least, will derive permanent advantages, carry away new ideas and also, and not least, pleasant recollections of this the first visit of the Museums' Association to Ireland.

III.

Recent Progress in Our Knowledge of Earthworms and their Allies.

THE present paper is intended rather to appeal to those who desire to be abreast of the latest information concerning a group of worms already provided with a voluminous literature, than to aim at a popular exposition of a subject hardly, perhaps, suited for such treatment. For some reason or other, the Oligochæta in general were, until a few years ago, perhaps, the least known group of any among the invertebrates; and yet they are particularly suited for investigation, and for many reasons. They are abundant, easily found, and easy to manipulate by dissection or with the microtome. Immense efforts are made yearly, at a large cost, to bring back a few skins and horns which are intrinsically as a rule of no interest at all, or of the very smallest importance. An infinitesimal fraction of the necessary expense incurred in the shooting and preservation of such material would supply many workers with abundant matter for study if devoted to the collecting and proper preservation of terrestrial and fresh-water invertebrates. To some extent this is now being done; and in the particular group upon which I report here, several collections of some importance have recently been made. Through the liberality of the Government Grant Committee of the Royal Society, I was able to secure the assistance of Mr. F. Finn to make collections in Zanzibar and Mombasa; more recently still, the Hamburg Museum sent out Dr. Michaelsen to South America, with the result that a very large collection of Oligochæta were brought home by him, and most kindly entrusted to me for study. Besides, the attention of naturalists travelling abroad with other aims has been successfully directed to the importance of this group; and in consequence a good deal of material has dribbled in to various Museums and individuals. Judging from the work of the last two or three years, which I shall endeavour to summarise here, we appear to be fairly advanced at the present moment in our knowledge of the main structural features of the group, at any rate of the terrestrial forms; the aquatic Oligochæta have been less collected, and are in a backward state so far as information goes. I shall now try to make a

connected story of recent progress, which will at least serve as a clue through the very widely scattered literature of the past year or two.

A good deal of attention has been paid to the phenomena of the distribution of these creatures, which are so important to the student of that branch of Biology. Apart from the notices of the occurrence of new species the most recent summary of that side of the study of the Oligochæta is to be found in no. 1. The difficulty which besets every person who attempts to philosophise upon the facts of distribution is to disentangle the really indigenous from the newly imported fauna. With earthworms this is apparently easier than with many groups. One may fairly speak of a "Holarctic" region embracing, of course, Europe, Northern Asia, and North America. The most prevalent genus is the common earthworm of this country (really consisting of many species), *Lumbricus*. There are somewhere about sixty species of this genus, considered *sensu lato* to include the very closely allied *Allolobophora* and *Allurus*. Nearly all of these are common to the Old and the New World; or, rather, but few of the *Lumbricus* of America are different from those of Europe. Now it is probable, in spite of the fact that this genus occurs most abundantly in gatherings from almost every other part of the world, tropics as well as the southern temperate latitudes, that the northern region is its true home. Two principal facts support this contention, which are as follows. The genus is most abundant in cultivated ground near the coast in the extra-Holarctic parts of the world; and, secondly, the species found are invariably identical with those of the northern temperate zone; the true indigenous earthworm fauna of South America, for example, has, as a rule, to be sought for outside the towns and their immediate suburbs. Were the *Lumbricus* truly indigenous to these parts of the world, one would fairly expect to meet with species different to those of the north; but one does not. Removing, therefore, this genus from the indigenous fauna of any countries save those enumerated above, it is easy to map out the globe into regions which, on the whole, correspond with those of Mr. Sclater. The one possible addition that might be made from this point of view to his six regions is an Antarctic form; but as I have treated of that matter in a quite recent number of this Journal, I leave it aside. Earthworms possess less facilities for active migration than many animals; a land-connection seems to be nearly absolutely necessary to their wanderings. It is perhaps otherwise with the aquatic Oligochæta, which are often, in correspondence with this, widely scattered. Vejdovsky (15) and I myself (2) have found that the so-called cocoons of the small freshwater form *Æolosoma*, distinguished by the beautifully and variously-coloured oil drops in the skin, are really chitinous cysts into which the worm temporarily withdraws itself. This is an evident assistance to involuntary migration, and it seems possible that some of the supposed "species" of this genus which have been described from Africa and America are identical with European species. The formation of

these cysts, in fact, doubles the animal's chances of being conveyed to distant places; the cocoon is, of course, the other chance. The cocoons of earthworms, being larger and often buried at considerable depths in the soil, are not likely to be accidentally attached to the feet of migratory birds, which, moreover, frequently affect the margins of lakes and pools, where the smaller cocoons of the aquatic Oligochæta would be very liable, one would imagine (there are no positive facts known to me), to be caught up and carried away. Another matter to be borne in mind is the more common occurrence here than among the earthworms of the escape of several young worms from one cocoon. The importation, therefore, of a solitary cocoon might serve to found quite a large colony in a new country. These are some of the facts and suggestions which arise from recent work upon the Oligochæta as regards the general phenomena of distribution.

But distribution has a bearing upon another branch of the subject which has received much attention—the classification of the group. The classification of the Oligochæta affords a literal instance of "Quot homines, tot sententiæ." The workers are, unfortunately, at present few, though this is mending, and everyone has his own individual scheme of arrangement; indeed, many of us have provided more than one scheme, though at fairly decent intervals. In attempting to decide upon the conflicting claims of different families to occupy a basal position in the series, it is reasonable to be influenced to a moderate extent by the facts of distribution. It is usually held that a wide and discontinuous range indicates an archaic form. Thus, for instance, *Peripatus* and the Tapir. Now, if we apply this to the earthworms it is perhaps the genus *Perichæta* which affords the most striking example. The genus has complete circles of setæ, from twenty to upwards of one hundred in number, according to the species, upon each segment of the body, and is found in Australia and India, Central America and the West Indies; it is feebly represented in tropical Africa and South America. The family Perichætidæ, may perhaps, be split up into four or five genera, but the differences are not great and there are gradations. The principal claim urged in favour of *Perichæta* as the most archaic existing form of earthworm was the condition of the excretory organs. In most text-books the typical Annelid excretory system is said to be a pair of tubes in each segment of the body, opening into the body-cavity by a ciliated funnel and on to the exterior by a pore. Researches, beginning with the year 1885, have shown that among earthworms a very large number of genera do not possess this arrangement at all. In *Perichæta*, for instance, that system is represented by a dense tufting of the body-walls and septa by innumerable coils of excretory tubes which open on to the exterior by innumerable pores scattered irregularly over the surface of the body. It has been stated by more than one naturalist that these nephridial tufts communicate from segment to segment, so that there can hardly be said to be any metamerism in an organ which in

so many Annelids is usually the most typically metameric. Bourne has, however, in his latest paper upon the earthworms of India, denied the connection from segment to segment. The matter is, therefore, still *sub judice*. From this diffuse and irregular network it has been held that the two pairs of separate nephridia per segment have been derived by reduction. Intermediate stages are offered by species with two and three separate pairs. This view of the origin of the nephridial system of the Oligochæta has, however, received a series of shocks at the hands of Vejdovsky (16), Bourne (10), and myself (3). We have all shown beyond any doubt that in the genera *Octochætus*, *Megascolides*, and *Perichæta*, genera in which the excretory system of the adult is constituted on the irregular, diffuse plan, the earliest appearance of this system is in the shape of a series of paired tubes, one pair to each segment. So far, therefore, *Perichæta* has no right to be regarded as a specially archaic form. It is, indeed, a matter of the greatest difficulty to adjust the rival claims of the existing families. More could doubtless be done if the exact relationships of the Oligochæta to other groups of "worms" could be definitely seen. In spite of the immense accumulation of anatomical detail during the last few years, but few facts which throw a light upon this most important question have been laid hold of. Something has been done to remove the barriers that were at one time supposed to divide the Oligochæta from the Leeches. The generally unpaired median male pores of the Eudrilidæ recall those of that group of Annelids; the continuity of the oviduct with the ovary is another character that removes one difference between the Leeches and the majority of the Eudrilidæ; finally, the same family of earthworms shows a considerable subdivision of the cœlom by the formation of sacs enclosing the spermathecæ and other parts of the generative apparatus; this is perhaps a foreshadowing of the almost obliterated cœlom, to speak with no great exaggeration, of the Leech. In two genera, viz., *Megascolides* and *Deinodrilus*, the dorsal vessel is enclosed in a sac forming a sort of pericardium, which is a step in the same direction. Even in histological details there are resemblances now known to exist which were formerly regarded as differences. Long ago Ratzel spoke of the leech-like muscular fibres of certain Oligochæta. The exact and beautiful researches of Cerfontaine (13) have proved that, in the common earthworm—and there is no doubt that the discovery applies to other Oligochæta—the muscular fibres consist of a softer granular core and of a radiately striate denser sheath which appears to be the exact counterpart of the muscular fibres of the Leech. This particular matter, however, takes us rather further. Quite recently Hesse (14) has pointed out that in the Enchytræidæ the muscles are more like those of a Nematode; he finds that, although there is apparently this ring of denser muscular substance surrounding a granular core, a gap exists in the said ring; through this split, often hardly recognisable owing to its fineness, the granular substance protrudes and is continuous with a large nucleated

cell. This accounts for the non-discovery of a nucleus within the muscular sheath of the fibre which was wanting to complete its likeness to the fibre of a Leech. A little more fanciful, perhaps, is my own comparison of the remarkable integumental network formed by the ducts of the paired nephridia, which ramifies through the body-wall of *Lybiodrilus* and more than one other Eudrilid, to the intra-integumental nephridial tubes of the Nematoidea; particularly to the coil of vessels which constitutes the Lemnisci of the Acanthocephala lately shown to exist in the true Nematoidea. In the Eudrilids in question the entire integument is riddled through and through with a complex system of tubes, all derived from the branching of the duct, by means of which the paired nephridia reach the exterior. There are, therefore, here as in *Perichæta* and some other forms, many external nephridial pores upon each segment of the body; but this same result is arrived at in a different way in the two cases. The most natural allies of the Oligochæta would seem to be the marine Polychæta; and yet recent researches have not tended to break down to any great extent the barriers separating these two groups. One character, however, hitherto held to differentiate the two has broken down. Cuvier called the Oligochæta as we now know them "Annélides abranches sétigères." In 1855, Grube described, unfortunately too insufficiently, a curious Annelid from the Nile which he called *Alma nilotica*; the same species has been lately re-discovered by Levinsen (12), who renamed it *Digitibranchus niloticus*. The worm has a series of tufted gills on the posterior segments. But it is not yet clear that the worm is not really a Polychæte. Apart from this dubious instance undoubted Oligochæta are now known to possess gills. Bourne was the first to discover this important fact; he found at Calcutta a Naid having a series of paired processes of the body-wall in the anterior segments. Later still I found in the "Victoria regia Tank" at the Botanical Gardens a worm allied to the common *Tubifex* of our streams and lakes with a series of dorsal and ventral gill-like processes upon the hinder segments; and more recently still (5) another Tubificid, belonging to a new genus *Hesperodrilus*, in which exactly the same kind of gills are present but lateral in position. Beyond these instances no Oligochæte has been discovered which places the Oligochæta nearer to the Polychæta than they were at the time when Claparède wrote. The phylogenetic arrangement of the Oligochæta must, therefore, still remain the aim of future investigation. The difficulties which surround the solution of the problem are not so much due to missing links in the chain as the inability to decide at which end of the chain was the beginning. Recent discoveries have rendered it in some cases almost an impossibility to distinguish family from family by any salient characters.

We may turn now to matters of special anatomical interest not having so great a bearing upon the inter-relationships and phylogeny

of the group. First among recent publications stands the monumental work of Vejdovsky (15) upon the embryology of *Rhynchelmis* and *Lumbricus*. One of his most important results is a clearing up of the very complicated series of excretory organs which arise at different times in the embryo. The first organ which serves this purpose is a series of tubes excavated in the interior of certain epiblast cells at the gastrula stage or even before; these cells are traversed by clear tubes which open behind into the primitive body cavity, *i.e.*, the space between the epiblast and the hypoblast. Next there is in some forms a pair of long tubes which open on to the exterior in the head region; these are the ciliated larval pronephridia, and they seem to be developed out of, or at least to have some connection with, the last-mentioned larval excretory cells. Quite independent of the last are the embryonal pronephridia; these organs are a series of paired tubes which open into the body-cavity by a "flame-cell." The flame-cell becomes a ciliated funnel, the nephridium grows, and we have the permanent nephridium of the adult. To give a thorough account of Vejdovsky's great book would take up more space than we can give; but one other point of some importance must be mentioned. He found that in the young *Lumbricus* the vascular system consists of a dorsal and of a ventral vessel which are connected by circum-intestinal vessels in each segment of the body; the dorsal vessel, as Vejdovsky himself was the first to discover a good many years ago, is at first double, a state of affairs which is permanently retained in a considerable number of earthworms throughout life. The connection of the dorsal and ventral vessels by a series of similar tubes in all the segments of the body is precisely what is met with in the simpler forms of Oligochæta such as the Tubificidæ. As still further tending to show that this is the primitive arrangement in worms which no longer show it, I may mention Bourne's discovery (11) that in *Paranais* the young buds have a full complement of these commissural vessels which are largely wanting in the adult.

Recent investigations into the anatomy of that very remarkable family the Eudrilidæ have progressed so considerably during the last few years, that from being one of the least known they have become quite one of the best known groups of Oligochæta. They have been referred to above as suggesting a link in the direction of the Hirudinea. Other points of interest have arisen. The most marked character of the family is the presence of large, usually unpaired, sacs which open on to the exterior and generally contain sperm. For this reason they have been called spermathecæ, though their anatomical structure is not precisely that of the spermathecæ of other earthworms. The investigations of Rosa (9) and myself (4) appear to have put beyond a doubt that these large sperm-holding sacs are derivatives of the septa which acquire an external pore; they can, therefore, have no morphological relationship to the spermathecæ of, say, *Lumbricus*, though an intimate physiological relation. But the interesting point

about these sacs is that we have a series of stages in the substitution of them for the true spermathecæ. In several genera of Eudrilidæ, *e.g.*, *Heliodrilus*; there is a single median spermatheca, apparently definitely corresponding to the true spermathecæ of other worms. This is wrapped in a sac which also communicates with the ovary by narrower passages. In *Hyperiodrilus*, the true spermatheca—in this case as in the last quite independent of the envolving sac—is greatly reduced in size. Finally, we have in the majority of Eudrilidæ no true spermatheca at all, unless the sac of mesoblastic origin opens on to the exterior by means of its exceedingly reduced representative. It seems clearly a case of the replacement of one organ by another physiologically similar, but morphologically distinct. The spermathecæ are the organs which receive the sperm during congress. It is, therefore, a most inexplicable fact that occasionally they open not only on to the exterior, but into the lumen of the gut. The fact was discovered a good many years ago in the Enchytræidæ, where it often occurs, by Michaelsen. I have found that in the North American genus *Sutroa*, first described by Eisen, exactly the same state of affairs unexpectedly exists. These facts may be commended to those who are still arduously labouring in the hope of finding gill-slits in some creature which is not a Chordate. The singular relationship between organs which would appear to have nothing in common, is enhanced in singularity by the fact that in an Eudrilid, *Paradrilus*, where the spermathecæ are of the mesoblastic type, precisely the same intercommunication is met with.

So much detailed research has been carried on of late by Benham, Horst, Rosa, Michaelsen, Eisen, myself, and others, that it is hard to compress into the limits of a short review like the present the results obtained. I confine myself, therefore, to such as are of a wider interest than to specialists. The last matter that I shall deal with, omitting a vast amount of rather less important though highly interesting work, is a curious case of change of function exhibited in the family Eudrilidæ, to which I have so often had occasion to refer. In this family, as in others, there are calciferous glands appended to the œsophagus, which secrete carbonate of lime. In Michaelsen's genus *Stuhlmannia*, and a good many of its more immediate allies, these glands are metamorphosed somewhat. They still open into the gut, but the lumen is greatly reduced, and does not by any manner of means extend through the entire gland. The gland is, in the main, solid, being built up of densely-associated cells, looking like peritoneal cells; the whole mass is permeated by a rich network of blood-vessels, as in the typical calciferous glands. Here and there, however, the cells in question have assumed the appearance of columnar gland cells; and when this modification has occurred, it is the invariable rule that what looks like the lumen of a tubular gland is occupied by blood. If there were not this singular change, one might be inclined merely to comment upon the fact that the glands, instead of being

formed of a much-folded membrane, had, as it were, melted into a solid ball. But the peculiar relation of the cells to blood-vessels seems to suggest a changed function, connected rather with the vascular system than with the alimentary. The whole matter is one of great interest, but would need for its proper discussion some space, which could not be fairly occupied by an article professing only to give a sketch of some of the more important recent advances in our knowledge of the structure of the Oligochæta.

The bibliography which follows is, of course, not exhaustive of the last two years; I have merely quoted those papers to which I have especially referred.

REFERENCES.

1. **Beddard, F. E.**—"Geographical Distribution." *Proc. Zool. Soc.*, 1893, p. 733.
2. ————————"*Æolosoma.*" *Ann. and Mag. Nat. Hist.*, Jan., 1892.
3. ————————"Embryology of *Octochætus.*" *Quart. J. Micr. Sci.*, xxxiii., p. 495.
4. ————————"*Lybiodrilus.*" *Ibid.*, xxxii., p. 539.
5. ————————"*Hesperodrilus.*" *Ann. Mag. Nat. Hist.*, Feb., 1894.
6. ————————"*Sutroa.*" *Trans. Roy. Soc. Edin.*, 1892, p. 195.
7. **Benham, W. B.**—"Classification." *Quart. J. Micr. Sci.*, xxxi., p. 201.
8. **Eisen, G.**—"*Sutroa.*" *Zoe*, vol. ii., p. 321.
9. **Rosa, D.**—"*Paradrilus.*" *Ann. k.k. Hof. mus. Wien*, vi., p. 379.
10. **Bourne, A. G.**—"Development of *Perichæta.*" *Quart. J. Micr. Sci.*, May, 1894.
11. ————————"Naids." *Ibid.*, xxxii., p. 335.
12. **Levinsen, G.**—"*Digitibranchus.*" *Vidensk. Med.*, 1889, p. 318.
13. **Cerfontaine, A.**—"Structure of *Lumbricus.*" *Arch. Biol.*, x., p. 327.
14. **Hesse.**—"Enchytræida." *Zeitsch. wiss. Zool.*, 1893.
15. **Vejdovsky, F.**—"Entwickelungsgeschichtliche Untersuchungen," *Prag.*
16. ————————.—"Development of Megascolides." *Arch. mikr. Anat.*, xxx., p. 552.

F. E. Beddard.

IV.

Nearctic or Sonoran?

DR. A. RUSSEL WALLACE'S article on the mammals and birds of the Palæarctic and Nearctic Regions in last month's issue of NATURAL SCIENCE,[1] will have been read with the greatest interest by all students of the geographical distribution of animals. The tables of genera common to the two regions, or peculiar to one of them, seem to tell strongly in favour of retaining those convenient divisions of Dr. Sclater, which, by means of Dr. Wallace's own classical work on distribution, have become household words among naturalists all over the world. Yet I venture to believe that the facts may be so read as to support another view of the zoological regions of the ex-tropical continents.

The only alternative to the retention of the Palæarctic and Nearctic Regions of Sclater which Dr. Wallace mentions is their union into a single Holarctic Realm, as suggested by Dr. Heilprin in his work on the "Distribution of Animals."[2] But in a recent memoir on North American mammals[3] (already referred to in NATURAL SCIENCE[4]), Dr. C. Hart Merriam advocates a different and more natural view. He would unite only the "Boreal" part of Sclater's Nearctic Region with the Palæarctic, leaving the rest as a separate region, to which he applies Professor Cope's term "Sonoran." This Sonoran Region he considers no mere borderland between the Northern and Neotropical realms, but a primary division characterised by the presence of the most distinctive North American mammals.

The Boreal division of Dr. Merriam roughly agrees with the Canadian sub-region of Dr. Wallace's "Geographical Distribution of Animals." He maps it, however, as extending southward along the three great mountain systems of North America—the Alleghanies, Rockies, and Sierra Nevada, in parts as a continuous tract, elsewhere in isolated patches, some of which reach as far south as Arizona, California, and Carolina. As might be expected, however, there is no decided line separating the northern and Sonoran regions; a

[1] Vol. iv., p. 433.

[2] London, 1887 (Int. Sci. Series).

[3] *Proc. Biol Soc. Washington*, vol. vii., 1892, p. 1.

[4] Vol. iii., p. 288.

"transition" zone is shown on the map, which in places attains a breadth of over 300 miles, wherein the two faunas overlap. This zone is mostly confined to the northern and mountain districts of the United States, but it invades the British territories to the west of Lake Winnipeg, to the north of Lakes Erie and Ontario, along the southern bank of the St. Lawrence, and again in Nova Scotia.

If we now examine the distribution of the seventy genera of mammals given by Dr. Wallace as inhabiting the Nearctic Region, we find that only eight *Canis*, *Otaria*, *Bos*, *Sciurus*, *Sciuropterus*, *Spermophilus*, *Hesperomys*, and *Lepus* can be fairly regarded as ranging indiscriminately over the Boreal and Sonoran districts. Of these only one, *Hesperomys* (the Vesper Mice), is peculiar to America, and, on referring to Flower and Lydekker's "Mammalia," it appears that this should be included in the Old World genus *Cricetus*. The Sea-Lions (*Otaria*), though not Palæarctic, occur in other Old World Regions.

All other genera of North American mammals can be referred to either the northern or Sonoran fauna, even though some of them overstep the limits of the transition zone.

The Boreal fauna of Dr. Merriam comprises thirty of Dr. Wallace's genera[5] — *Sorex*, **Neosorex*, *Urotrichus*, **Condylura*, *Ursus*, *Lutra*, **Latax*, *Mustela*, *Gulo*, *Trichechus*, *Phoca*, *Cystophora*, *Cervus*, *Rangifer*, *Alces*, *Ovis*, **Haplocerus*, **Ovibos*, *Tamias*, *Arctomys*, *Castor*, **Haploodon*, *Arvicola*, *Myodes*, *Cuniculus*, **Phenacomys*, **Fiber*, **Zapus*, **Erethizon*, and *Lagomys*, of which only the ten marked by an asterisk are peculiarly American. Of the remaining twenty, all but six: *Ursus*, *Lutra*, *Mustela*, *Cervus*, *Ovis*, and *Arvicola* are only elsewhere to be found in the Palæarctic Region, while, even of these six, *Ursus*, *Ovis*, and *Arvicola* are characteristic northern genera, which can only be regarded as incursors in the tropical regions where they occur. These facts seem to point strongly to the affinity of the northern faunas of the Old and New Worlds, and to require the union of Boreal American with the Palæarctic Region of Sclater. It will be possibly urged as an objection that Boreal America is admitted to be the home of ten distinctively American genera. But if the relationships of these be examined, it will be found that only two, *Condylura* and *Erethizon*, are American in their affinities. The nearest relations of all the rest must be sought in the Old World. And *Condylura* and *Erethizon*, though confined to the Boreal *Region*, should be regarded as belonging to the Sonoran *fauna*.

If the genera of mammals already enumerated be compared with Dr. Wallace's lists, it will be seen there are thirty-two remaining, which are fairly reckoned by Dr. Merriam as distinctively Sonoran, although nine of them range into the southern parts of the Boreal division. Of these thirty-two, only five, *Plecotus*, *Vesperugo*, *Vespertilio*,

[5] The genera adopted by Dr. Merriam are more favourable to his views than those given by Dr. Wallace. I take, however, the latter as the basis for comparison.

Nyctinomus, and *Felis* occur also in the Palæarctic Region. The remaining twenty-seven are peculiarly American, and it will be seen that this number is just over two-thirds of the thirty-nine given in the third column of Dr. Wallace's table as the number of Nearctic genera not to be found in the Old World. The peculiar family of the Pouched Rats (Geomyidæ) is specially noteworthy as coming distinctly into the Sonoran fauna, only one of its genera (*Thomomys*) just entering the Boreal Region. The other peculiar mammalian family, Antilocapridæ (Prong-horn), is also seen to be typically Sonoran. The differences between the two mammalian faunas in North America may now be shown in tabular form :—

	Distinctively American.	Common to America and Palæarctic Region.
Genera common to Boreal and Sonoran Regions ..	2[6]	6
,, typically Boreal ..	10	20
,, ,, Sonoran ..	27	5

And it becomes clear that the affinities of the northern fauna of America are with the fauna of Europe and temperate Asia, while the Sonoran fauna is exceedingly distinct.

When we turn to the birds we find, as might be expected, that the regional divisions are not so closely kept as by the mammals. Nevertheless it is possible, by a survey of the distribution of each genus, to determine to which fauna it belongs. A genus with several Sonoran species and but one Boreal may fairly be reckoned as Sonoran. In compiling the following summary I have taken the distribution as given in the A.O.U. Check List, whence Dr. Wallace compiled his genera :—

	Distinctively American.	Common to America and Palæarctic Region.
Genera common to Boreal and Sonoran	30	32
Typical Boreal genera ..	2[7]	14[8]
,, Sonoran ,, ..	81[9]	8[10]

It is not necessary to give lists of all these genera. I give in the footnote their reference numbers in Dr. Wallace's lists, which will enable anyone interested in the subject to check my results.

It will be seen that of the hundred and thirteen genera of birds which, according to Dr. Wallace, differentiate the Nearctic from the

[6] One of these (*Otaria*) is not *peculiarly* American.

[7] Genera nos. 34, 35 (3rd col.).

[8] ,, ,, 18, 20, 21, 22, 23, 25, 26, 27, 31, 36, 37, 47, 53, 54 (2nd col.).

[9] ,, ,, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 15, 16, 17, 18, 19, 23, 26, 28, 32, 33, 37, 41, 42, 46, 47, 48, 49, 50, 51, 52, 53, 54, 55, 56, 57, 59, 61, 62, 63, 64, 65, 66, 67, 68, 70, 71, 72, 74, 75, 77, 78, 79, 80, 81, 82, 83, 85, 88, 89, 90, 91, 92, 93, 94, 95, 96, 97, 98, 99, 100, 102, 103, 104, 105, 106, 107, 108, 109, 111, 113 (3rd col.).

[10] ,, ,, 6, 7, 8, 29, 33, 41, 49, 52 (2nd col.).

The sixty-two genera not enumerated here are common to the Boreal and Sonoran tracts.

Palæarctic Region, eighty-one, or over 70 per cent., are typically Sonoran, while only two are typically Boreal. That as many as thirty genera should range over the whole continent may be urged in support of the retention of the Nearctic Region intact. Yet many undoubtedly Neotropical genera range into the Sonoran; and when we consider the great extent of the North American continent and the absence of any natural barrier to check migration from south to north, our surprise should be that the mixing of the two faunas has not been more complete.

The second column of the summary given above yields highly suggestive facts. With the single exception of *Astur*, all the fourteen typical Boreal genera common to North America and the Palæarctic Region are confined to the latter region in the Old World. This shows most strongly the affinity between the Boreal American and Palæarctic bird faunas. On the other hand, we find that of the remaining forty genera in the second column, only three (*Acredula*, *Nyctala*, and *Archibuteo*) are, in the Old World, typically Palæarctic. The rest are either of very wide range, or occur in both the Palæarctic and Neotropical Regions.

Strong confirmation of the distinction between the Boreal and Sonoran faunas in America can be obtained from other groups. The paucity of Lizards in the former fauna has indeed been well shown by Dr. Wallace himself, as a survey of the tables of distribution of families in the second volume of his great work shows that but one genus of these reptiles enters his Canadian sub-region. This is *Gerrhonotus*, placed by Mr. Boulenger in the latest British Museum Catalogue in the Family Anguidæ. On looking through that Catalogue I find, on the other hand, that the Sonoran Region is characterised by no fewer than 22 genera of lizards:—*Eublepharis*, *Anolis*, **Dipsosaurus*, **Sauromalus*, **Crotaphytus*, **Petrosaurus*, **Callisaurus*, **Uma*, **Holbrookia*, *Uta*, *Sceloporus*, *Phrynosoma*, *Gerrhonotus*, *Ophisaurus*, **Anniella* (forming a peculiar family), *Heloderma*, **Xantusia*, **Cnemidophorus*, *Chirotes*, **Rhineura*, *Lygosoma*, and *Eumeces*, of which the 11 marked with an asterisk are not found elsewhere. The genera which are not peculiar have either Neotropical affinities or are represented in corresponding latitudes in the Old World. The Tortoises tell the same story. A survey of the land and fresh-water genera of Chelonians, as given in Mr. Boulenger's British Museum Catalogue, shows that only two genera, *Trionyx* and *Emys*, neither peculiar to America, reach the Boreal [11] district, while the Sonoran, in addition to these, possesses *Chelydra*, *Macroclemmys*, *Cinosternum*, *Chrysemmys*, **Malacoclemmys*, *Clemmys*, **Cistudo*, and *Testudo*, the two distinguished by the asterisk being peculiar. The Sonoran Region is further to be distinguished by the genus *Alligator*, among the Crocodiles.

[11] The former, indeed, seems not to pass Dr. Merriam's transition zone.

While admitting the great convenience of the Six Regions of Dr. Sclater, it seems that the above-mentioned facts yield strong support to the suggested revision of their boundaries. The addition of the Boreal tract of North America to the Palæarctic Region will render that great division of the earth's surface more natural than it is at present, as the elements of the Northern American fauna are seen to be chiefly Palæarctic in their affinities. To this great realm Dr. Heilprin's name of Holarctic might be applied, or it might perhaps be better known as the Great Northern Region. I need hardly remind students of Geographical Distribution that such a region was mapped thirty years ago in Murray's work[12] on Mammalian Distribution. The Sonoran Region will, indeed, be somewhat less rich than the old Nearctic in peculiar forms of life, but it will retain its most distinctive characteristics and comprise a more natural fauna than did the larger tract. The objection to the new arrangement, which I cannot but feel to be considerable, is that, even outside the broad transition tract of Dr. Merriam, overlapping of the two faunas takes place, especially in the extension of peculiarly American birds to the Arctic Ocean. Still, I cannot think that this overlapping should deter us from adopting the more natural division. No zoological region can be mapped with the hard and fast line of a political frontier, and the zoologist must always think more of faunas than of geographical boundaries. That two distinct faunas exist in North America seems abundantly clear, although the nature of the country has allowed considerable overlapping. It is well to remember that in parts of the Mediterranean sub-region there is a decided Ethiopian colouring to the fauna, in spite of the barrier of the Sahara. Were Africa without that barrier to the northern spread of Ethiopian forms, we should have such a mingling of two faunas as would necessitate a wide transition tract on our own side of the Atlantic.

GEO. H. CARPENTER.

[12] The "Geographical Distribution of Mammals." By A. Murray. London, 1866.

SOME NEW BOOKS.

The Art of Stuffing.

Scientific Taxidermy for Museums (based on a study of the United States Government collections). By R. W. Shufeldt, M.D. Smithsonian Institution. Report of the U.S. National Museum for 1892, pp. 369–436, pls. xv.–xcvi. Washington, 1894.

It was the Rev. J. G. Wood who once said that his earliest attempt at stuffing an animal was made upon a mouse. After skinning the creature, he cut off so many inches of his school ruler as would fill up the interior, and stitching together the margins of the skin under the belly, found a satisfaction in contemplating the success of his creation. The old days of monstrosities are rapidly fading away in all the larger institutions, and especially in our own British Museum of Natural History. No longer do we there see those highly polished cattle in whose sides, rumour has it, it was quite possible to get enough reflection to arrange one's hair; and we may even discount the statement of certain evilly-disposed persons who affirm that within recent years there was to be seen one animal in the British Gallery whose flanks had been repaired by a process of skin grafting of a somewhat unsuccessful nature. The very beautiful examples of individual stuffed animals, or groups of animals, which are fast displacing their old and worn representatives in our National Museum are too well-known to need recalling here, and Dr. Shufeldt refers to them as "Perhaps some of the finest groups in the world." Two of the best of these groups (of birds) Dr. Shufeldt has been able to reproduce through the courtesy of Dr. Bowdler Sharpe, and in doing so he pays a high compliment to "that distinguished ornithologist's" interesting paper on these stuffed groups which appeared in the *English Illustrated Magazine* for December, 1887.

In an exceedingly interesting sketch of the history of taxidermic art, prefixed to the paper, Dr. Shufeldt reminds us of Hanno's discovery of the gorilla five centuries before Christ. These were flayed, and the skins taken to Carthage, where they were preserved for many generations, and described by Pliny as Gorgones. He also refers to the robes of the Mexican kings, with their gorgeous ornamentations of trogon and other brilliantly plumaged birds; quotes Shakespeare's apothecary, within whose

> Needy shop a tortoise hung,
> An alligator stuffed, and other skins
> Of ill-shaped fishes,

and finally tells us that the earliest stuffed specimen, as opposed to a mere skin, now known to exist, is a rhinoceros still preserved in the Royal Museum of Vertebrates in Florence, and dating from the sixteenth century.

Dr. Shufeldt next discusses the methods employed by the best taxidermists of the century, and points out the advantages of a liberal education and the importance of a first-rate knowledge of general

biology and anatomy. The taxidermist should be, besides, a good field naturalist, a careful observer of nature and of the habits of animals, a botanist, a topographer, and an artist. The importance of photography is fully insisted upon, and Dr. Shufeldt considers a perfect knowledge of the art and practice of photography more useful perhaps than anything else. He says: "No taxidermist who has any regard for an attainment of excellence in his calling should neglect to make good photographs of all the living animals that he can, and that upon every possible opportunity. This should not be confined to wild animals alone, but to all the domestic ones in their most common attitudes."

Passing on to the true object of his paper, the criticism and explanation of stuffed or modelled specimens of natural history objects, Dr. Shufeldt refers to the beautiful gelatine casts, taken from plaster moulds, of the octopus, squid, and other soft-bodied invertebrates, shown at the Chicago Exposition, photographs of which he reproduces in support of his words. Noting the difficulties of the proper preservation of fishes, he mentions the remarkable collection of preserved fish in the Government Museum at Madras, which Hornaday referred to as the finest of its kind in existence. The lamentable aspect of fishes in alcohol and the further distortion produced by the jars in which they are usually preserved is noted. The advantage of making plaster casts, properly coloured, is insisted upon, and emphasised by photographs of some beautiful examples in the U.S. National Museum. The "Buckland" fishes will be remembered by most readers of this article, and the successful preparations made by Mr. Montagu Browne in more recent years. In our own Museum at South Kensington, we see an attempt to replace the ordinary museum fish with some more lifelike stuffed examples, but the use of plaster or the Browne methods might be more largely employed to advantage in both the Zoological and Geological Galleries. Dr. Shufeldt says, speaking of rays, that the plaster and gelatine methods are so successful with these fish that, properly coloured, it would be difficult to distinguish between photographs of the living fish, the plaster, and the gelatine cast, if they were all taken on the same scale and under the same conditions. In all his statements, too, the reproduction of photographs of fish-models fully bear him out.

Some half-dozen photographs of beautiful plaster casts of snakes are reproduced, of which the rattlesnake, in a hole in the rocks, is wonderfully true to nature. Indeed, in these animals it is difficult to see why anything else but plaster casts of excellence should be shown in public galleries, so true is the cast, and so difficult is it to properly stuff the creature itself. The reproductions of photographs of stuffed birds have by far the largest space allotted to them, and they form an exceedingly interesting collection. Examples are given of correctly and incorrectly stuffed auks, gulls and penguins, of a scolding parrot, a parrot on the defensive, of game cocks and hens, a pigeon preening the breast feathers, a quail elevating feathers prior to shaking, and also in the act of walking, of owls, and various groups of birds, including, as before mentioned, some in our own Museum, of rare excellence.

In the Mammalia, examples are given of kangaroo properly and improperly stuffed, a group of armadilloes, a porpoise, gazelles, a group of *Bison americanus*, the head of a zebra of remarkable excellence, jack rabbit, head of a tiger, and many more which we are unable to refer to.

In writing this paper, which we specially commend to the notice of the Museums' Association which meets this month at Dublin, Dr. Shufeldt has earned the gratitude of every true student of nature, he has done the taxidermist an incalculable service in elevating his work from that of a mere stuffer to that of an artist, and there is no doubt he will see his closing words realised in "the further encouragement and stimulation of the progress of the art of taxidermy."

More Extinct Monsters.

Creatures of Other Days. By the Rev. H. N. Hutchinson. 8vo. Pp. xxiv., 270 (illustrated). London: Chapman & Hall, 1894. Price 14s.

In a previous issue we had the pleasure of noticing Mr. Hutchinson's first literary and pictorial attempt at the restoration of extinct animals, and then expressed the hope that the work in question would soon reach a second edition. Not only has this hope been realised, but "Extinct Monsters," as the first volume was entitled, has been so successful from a publishers' point of view that it is now followed by another work of a similar nature. While carefully avoiding all mention of invertebrates in this new venture, the author takes into consideration several groups, such as the fishes of the Old Red Sandstone, the primeval salamanders, and the anomodont reptiles, which were altogether passed over in "Extinct Monsters"; while the treatment of many of the groups noticed in the latter is expanded and amplified. That Mr. Hutchinson has produced a book which forms a worthy companion to his first effort, we are fully assured, and we can only hope that it may obtain as much appreciation from the general public as seems (deservedly) to have fallen to the share of the latter. So far as the illustrations are concerned, we think that the book is almost beyond praise, and we notice that in his restorations of the dinosaurs the artist has considerably improved on his former efforts. Much interest will undoubtedly attach to the figures of the primeval salamanders or labyrinthodonts, as well as to that of the strange and gigantic pariasaur from South Africa. We have too long been accustomed in text-books to see labyrinthodonts restored in the form of impossible frog-like monsters; and it is refreshing to find them now in a guise somewhat near to what was probably their living form. The restoration of pariasaur strikes us as particularly successful, but may we ask the author why, on page 81, he transliterates the name of this creature as *Pareiasaurus*, while, on the preceding page, we find a name which has a similar diphthong in the original spelt *Tapinocephalus*? We may also suggest that he is not quite up-to-date in his treatment of the anomodonts and their allies, among other errors including *Proterosaurus* in that group, whereas it has been pretty conclusively shown to be a rhynchocephalian. Indeed, one of the chief faults we have to find is that the author, both in classification, nomenclature, and anatomy, follows one or two palæontologists to the exclusion of others.

On the whole, the book is written in a pleasant and agreeable style, although sometimes, as in the first paragraph on page 241, some of the sentences appear to have no relation to those which precede them, while in other cases the style is not free from grammatical faults. Perhaps the author will have the goodness in a second edition to inform us whether the sentence on page 107, stating that "like the gavials of India, they [Liassic and Oolitic Crocodiles] doubtless swarmed in the rivers and lakes, and preyed on the fishes that lived in the seas of those periods," is to be attributed to such a slip in grammar, or whether he possesses a

knowledge of the former distribution of Indian gavials which is denied to humble individuals like ourselves. Two pages later, we meet with the astounding statement that a "genus probably attained a length of ten or twelve feet." May we ask how you measure a genus, as this is another point in which we confess to a pitiful state of ignorance? Again, on page 212, we notice that the genus *Paloplotherium* is stated to be on the ancestral line of the horse, and to be very similar to *Hyracotherium*. We have, hitherto, been misguided enough to believe that the animal known as *Paloplotherium* was identical with the one mentioned on page 180 as *Palæotherium minus*, and that it had nothing on earth to do with the descent of the horse. We must, however, presume that the learned author is here also enabled to correct by his knowledge the ignorance in which we have hitherto been enveloped. The statement on page 112, that these and other creatures "are all nearly related to a previously known but larger animal, called by Cuvier *Lophiodon*," likewise reads to us somewhat curiously, seeing that the genus *Lophiodon* includes about a score of species, ranging in size from a sheep to a big rhinoceros.

Although the limited state of our knowledge, in comparison with that apparently possessed by the author, precludes our criticising such statements with the authority which is desirable, we are not hampered by any such disabilities when we point out manifest contradictions in the book. Turning, for instance, to page 130, we read that, "it has sometimes been asserted that birds were derived from dinosaurs in the course of evolution, but this is not a correct statement of the views of leading naturalists such as Professor Huxley"; but a little later (pp. 151, 152) it is written that "we have already alluded to Professor Huxley's theory that birds are descended from dinosaurs." This is unkind, as it would have been a source of great gratification to us to have learnt what the professor's views on this interesting subject really are. Be they what they may, the author is, however, by no means prepared to give them his unqualified assent, and accordingly draws upon his own stores of knowledge of the anatomy of the two groups in question in order to disprove them.

Before leaving the author's style, may we suggest to him that it would be much less irritating to ourselves, and probably to other readers, if, when mentioning the names of the authorities referred to, he would refrain from appending certain mystic letters to their names? Such additions only spoil the look of the text, and, although it is just conceivable that it may gratify a small percentage of the personages thus honoured to see such additions to their names, to the ordinary reader it is a matter of the most supreme indifference whether they are entitled to be called F.G.S., F.R.S., L.C.C., or P.C.L.P. (parish councillor for Little Pedlington!); and on this subject we commend Lowell's "Note to Title-page" of the "Biglow Papers" to the author's notice. We cannot, moreover, pass over the statement on page 18, to the effect that in England "so little is done for the cause of science by our rulers." In the first place, in this democratic age it is not "our rulers" but ourselves who are responsible for the amount spent on science; and, secondly, in our own opinion, considering the very small percentage of the population that takes an interest in Zoological Science, quite sufficient money is expended by Government thereon. Personally, as a taxpayer, the writer would be most unwilling to contribute a single farthing more towards such a subsidy.

Reverting for a moment to the Labyrinthodonts, we confess that

THE HIPPARION (UPPER PICTURE), AND A RECENT WILD HORSE (LOWER PICTURE). (From *Creatures of Other Days.*)

RESTORATION OF THE DINOTHERIUM. MIOCENE PERIOD. (From *Creatures of Other Days.*)

we are somewhat puzzled by the statement on page 13 that in the figure of the tracks on the opposite page only one-half of the track of the animal is delineated. Surely, as the impressions are both right and left they must include those of the feet of both sides; if not, will the author kindly explain his views more fully?

The restorations of the mammals are for the most part well done, and they are probably as near like the originals as we are ever likely to approximate, although it is probable that even the artist himself would be willing to admit that the indications of colouring, which have in some cases been attempted, are more or less fanciful. It is, moreover, a little misleading to find pig-like creatures included in the chapter entitled "Tapir-like Animals, etc." That the book will attract a large number of readers, we have little doubt, and we cannot do more than wish it a success similar to that which has attended its predecessor.

The Cult of the Camel.

The Camel: Its Uses and Management. By Major A. G. Leonard. 8vo. Pp. ix. and 335. London: Longmans & Green, 1894. Price 21s.

It is a well-known fact that during the last Cabul war and the Nile Expedition the loss of camels through overwork, improper or scanty feeding, and bad treatment, was almost appalling; and although a considerable proportion of this loss was inevitable, there is little doubt that the number of casualties might have been enormously reduced by the bestowal of more attention on the needs and habits of the animals in question. As wars in the same regions or other districts where camels can be employed are inevitable in the future, we heartily welcome Major Leonard's attempt to give Tommy Atkins and his superiors some insight into the habits and needs of these invaluable although unsociable creatures. As the author was both present during the Afghan war and accompanied the Camel Corps during the march to Korti, where he appears to have been connected with the commissariat department, his opportunities of observing the camel at work have been unrivalled; and that good use has been made of such opportunities, the present work affords ample proof. In the book before us we have chapters on the characteristics and temperament, and the instinct and intelligence, of camels, as well as others on their breeding, watering, feeding, loading, management, and ailments, and further one on their capacity and endurance. For the most part, these are well written, and ought to show any intelligent officer how to make the best use of these animals when put under his charge. We notice that, although the stupidity of the camel is admitted, Major Leonard does not give the camel such a bad character in general as is attributed to it by Palgrave and others; and he is doubless right in his assertion that much of the moroseness of its disposition is due to bad treatment and mismanagement. Attention may also be directed to the author's observations on the importance of watering camels at stated intervals whenever practicable, since there is no doubt that during the wars alluded to above a large proportion of the deaths were due to the mistaken belief that these animals are capable of going for an almost unlimited period without water.

If the author had confined his attention to the camel as a beast of burden, we should have had nothing but commendation for his book; but he has unfortunately thought it necessary to say something of the geological and zoological history of the creature, and in the course of his observations renders the statement in the introduction that he is

not a naturalist altogether superfluous. It strikes us, for instance, as somewhat curious to find (p. 1) the camel referred to as one of the oldest living mammals, while a few lines further on it is stated that its prehistoric ancestors are unknown. The context shows that this statement is due to Andrew Murray's belief in the close alliance between the Siwalik and modern camels; but if the author had consulted authorities on the subject he would have found that they are very different, and his confusion between a species and a genus is too obvious to need pointing out. When, moreover, the author (p. 2) states that "it is a curious fact that the former description [? distribution] of the family seems to be much wider than the natural habitat of the forms now extant—a fact which may, perhaps, be attributed to the Deluge, and the sundering of what in those days were vast globes into smaller and insulated continents," he is not only a bit behind the times, but likewise most remarkably enigmatical. Still more startling is the assertion on the same page—made on the authority of a South African friend who has *compared the figures* —that the *Helladotherium* of Pikermi, instead of being, as we have all supposed, a near cousin of the giraffe, is nothing more than a camel! What will our friend Dr. Forsyth Major think of this as the result of all his labours to instruct the public? Other instances of misconception and error might be referred to, but we can only call attention to the assertion, on p. 22, that a camel has twelve molars on each side of the upper jaw, and ten in the lower, as a sample of the anatomical blunders disfiguring what is otherwise an excellent work.

Blind Crustacea.

The Subterranean Crustacea of New Zealand: with some general remarks on the Fauna of Caves and Wells. By Charles Chilton, M.A., D.Sc. *Trans. Linnean Soc. London*, ser. 2, Zoology, vol. vi., pt. 2, May, 1894.

This paper treats of the curious blind crustacea which inhabit the underground waters of the Canterbury Plains in the South Island of New Zealand. The author leads off with an historical account of previous writings on the subject, and gives a fuller bibliography at the end of his paper. Detailed descriptions of the six New Zealand forms follow, and the author then passes to the more general considerations which we present as interesting to the majority of readers. One of the characteristics of these underground crustacea is the absence of colouration. The integument and greater part of the body is colourless, and though traces of colour (pink or yellow) are occasionally observed, this is the result of the yolk of the egg in females, the liver tubes, or the red colour of the fluid in the vascular system (in worms). In comparing these subterranean forms with deep-water organisms which often show a more or less definite colouration, the author is inclined to believe that the difference in colour is due to some differences of chemical composition rather than to the presence or absence of light. Professor S. I. Smith has previously stated that the darkness existing beneath 2,000 fathoms of water is very different from that existing in ordinary underground caverns.

With regard to eyesight, Mr. Chilton observes that in the New Zealand specimens he has been unable to find any external trace of eyes except in one species, *Crangonyx compactus*, in which the eye is represented by two or three imperfect lenses apparently quite without pigment. He has as yet had no opportunity of making sections to study the conditions of the optic lobes and nerves. On the whole, too, the New Zealand forms give only a modified support to the conclusion

that the subterranean species are more abundantly supplied with sense-organs (other than eyes) than allied surface animals. He is unable to add any fresh information as to the food and food-supply of these crustacea. With regard to arrested development previously noticed among these animals, Mr. Chilton says he has a good example in *Cruregens fontanus*, which has the seventh segment of the peræon small and without appendages, as is the case in the young forms of many Isopods. Of the habits of *Cruregens*, he notes that "they ran backward and forward with equal rapidity, and did not seem particular which way they went; they did not swim, and if they dropped off a plant wriggled helplessly till they reached the bottom." They ran against objects in a way which seemed to indicate that they were totally blind, and he occasionally saw two approach very near each other, apparently without being aware of it, and then suddenly jump apart when they touched.

Speaking of the age of the cave fauna of New Zealand in particular, the author notes that all the places where subterranean forms are found are marked on Professor Haast's map as either "post-Pliocene alluvium" or "recent alluvium" and mostly in the latter, and he suggests that it is quite possible, if thorough search were to be made, that some species of the genera (especially of *Phreatoicus*) would still be found inhabiting fresh-water streams among the Southern Alps of the country. The paper is well illustrated by some quarto plates, and is a valuable contribution to a most interesting subject.

A Criticism and a Theory.

Gestaltung und Vererbung. Eine Entwickelungsmechanik der Organismen. By Dr. Wilhelm Haacke. Pp. 337, twenty-six illustrations in the text. Leipzig: T. O. Weigel Nachfolger, 1893. Price 8 marks.

Although Dr. Haacke does not proceed to the display of his own theory until he has spent over one hundred pages in criticising the theories of others, and specially the theory of Weismann, it may be more convenient to attempt at once to give an account of this new panacea for all biological difficulties. His theory is *epigenetic* in the strictest sense. He believes that the plasma of the whole body of each animal and the plasma of the germ-cells are of the same kind. Hence he thinks that the shape and symmetry of the adult must be explained by the shape and symmetry of the elements in the plasma of the germ-cell from which the adult grew. The plasma is made up of ultimate elements, which Dr. Haacke terms "*gemmæ*." These are rhombic prisms, and the process of assimilation consists of the formation of new *gemmæ*. The unformed food of the plasma is built up into the peculiar *gemmæ* of the particular animal, or plant, in some such fashion as inorganic particles grow into crystals under the stimulus of the presence of crystals of their own order. *Gemmæ*, by the adhesion of their faces, are built up into units of a higher order, called *gemmaria*. As out of one set of similar bricks many different castles, depending on the number and arrangement of the bricks in each castle, might be formed, so out of a comparatively small variety of *gemmæ* an indefinite number of *gemmaria* might be obtained. It is these *gemmaria* that are the real morphological units. Out of one *gemmarium* you could tell the whole animal. But as from their size, if from no other reason, the investigator is exceedingly unlikely ever to see the *gemmarium*, Dr. Haacke goes to work the other way about, and endeavours to deduce, from the shape of adult organisms, the shape of their *gemmaria*. This he does in a sufficiently

ingenious manner, and at least succeeds in convincing the reader that the shapes of animals are not altogether unprofitable subjects of study.

The architecture of the *gemmaria*, as one learns from Dr. Haacke's descriptions and figures, is of a simple kind. They form no airy structures with arches and beams and colonnades. The *gemmæ* are superimposed in solid masses as our primeval forefathers might have built monuments to their fathers, or altars to their gods. As there are different numbers of *gemmæ* in the successive tiers, they resemble still more closely the solid staircases which we, as children, formed of our wooden bricks.

These *gemmæ* may adhere together closely or loosely. In each generation individuals with *gemmaria* of which the *gemmæ* are closely adherent are very naturally more stable than those whose *gemmaria*, so to speak, dissolve with every puff of wind. This is Dr. Haacke's interpretation of differences in the strength of individual constitutions. He points out that, in the struggle between individuals, it is not the individual with a claw slightly longer, or with a tooth slightly sharper, that, on the average, survives longer to propagate his species more largely. It is the individual generally better endowed, generally more adapted to resist every blow of fate, to take advantage of good fortune of every kind, that is favoured in Nature's battle. Interpreted into his theory, this means that in each generation those individuals with little adhesion of the *gemmæ* in the *gemmaria* speedily are discomfited; that, in fact, there is a gradual phylogenetic strengthening of the stability of the *gemmaria*.

From this another result ensues. Strengthening of the *gemmaria* implies a resistance to change of any kind. Just as the stable *gemmaria* resist external influences that might be to their hurt, so they resist influences that might be to their advantage. Thus, with age each race becomes hardened. It pursues its own line of progress resisting all other influences, and so a race may die out, as many races seem to have died out, because it persists in a definite line of development after that line has ceased to be of advantage to it, and has even become detrimental.

The plasma, that is to say the *gemmaria*, all over the body of an animal is not only similar in shape and structure, but is in a condition of organic stability. Hence it follows that any external influence acting on a part of the organism, so as to alter the shape of the *gemmaria*, will disturb the equilibrium of the whole body. The disturbed system gradually settles into a new position of stability, a position in which the new force has been registered. Thus it happens that every change of any part of the organism is reflected upon the plasma of the germ-cells of the organism. Thus, acquired characters are inherited, subject, of course, to the limitation that the impacted force must be sufficiently great, or last through a space of time sufficiently long to counteract the stability of the *gemmaria* acquired in ages of elimination of the less stable. Thus, for instance, Dr. Haacke scoffs at the attempt of anyone in a few generations of mutilations, by docking the tails of even 90 generations of mice, to prove or disprove the question of the inheritance of acquired characters. When nature has been building up *gemmaria* that give rise to tails, and hardening these *gemmaria* for a million generations, it is ridiculous for man with his scalpel and his few score years to hope to interfere.

It may be possible that, to some readers, a theory so purely theoretical as this theory of Dr. Haacke's may not be attractive.

Even such we advise to peruse Dr. Haacke's book. For running through this warp of ingenious, but possibly perverse speculation is a woof of actual observation, of shrewd criticism and valuable suggestion. So far as we are concerned we do not think that this theory has the slightest merit even as a provisional hypothesis. We think it the purest moonshine, and regret that a man of Dr. Haacke's evident ability should have published it, even as a jest. This opinion notwithstanding, we think that there are very few pages in the book where a biologist will fail to find a novel handling of novel facts and a most suggestive and stimulating treatment of the most interesting questions of biology. There are among us many given to the translating of strange books. If the idea of translating this book occurs to anyone, we offer the following advice: Let him carefully eliminate every reference to the particular theory of the *gemmaria*; let him curtail a few of the pages of algebraic formulæ, and the volume, so trimmed and lightened, will prove as useful, attractive, and valuable as any book dealing with biological theory published since Darwin died.

Flora of France.

Flore de France, contenant la description de toutes les espèces indigènes disposées en tableaux analytiques et illustrée de 2,165 figures représentant les types caractéristiques des genres et des sous-genres. By A. Acloque. 8vo. 1 vol. of 816 pages. Paris: J. B. Ballière et Fils, 1894. Price f.12·50.

In a laudatory preface, which takes the form of a letter to the publishers, M. Ed. Bureau, professor of botany at the Natural History Museum in Paris, expresses his surprise that anyone should have been found sufficiently bold to undertake a work so considerable as that which M. Acloque has just accomplished. Youth only, he says, could have been capable of so daring an act, but *audaces fortuna juvat*, and fortune has not failed the author in the present case, for M. Bureau finds that the book answers the only sure test which can be applied to one of its kind, namely, to enable the student to run down to their specific names several plants taken at hazard. The object of the work is to assist the field botanist to determine his plants by the aid of dichotomous keys after the manner, for instance, of Hayward's botanists' pocket book, though M. Acloque's bulky volume would make about four of these as regards size. Besides the key the distinctive characters of the families are also given, arranged in sytematic order. In the analytic table of the families the student has the assistance of numerous figures which the author reminds us must be considered as diagrammatic. They will doubtless be useful, though some of them are so extremely rough that they were better left out altogether. For instance, the first four, which are to illustrate vascular and three forms of cellular tissue, will be more of a hindrance than a help to one who is ignorant, while he who knows will scoff at them. The diagrams, however, generally answer well enough to indicate the form of organs or their appearance in section. In the elucidation of genera and species numerous figures are given to illustrate the habit of the plant or inflorescence, and sometimes the form of the leaf, flower, or fruit. Some of these are better than others, but as a whole they compare unfavourably with the plates in Bentham's illustrated handbook. The mere fact that there is in France no similar work will ensure the book a welcome; the text, however, is well arranged and will, except in failing light, when it will be found trying to the eyes, prove a great boon to the student of field botany.

The Natural History of Plants.

The Natural History of Plants. By Kerner von Marilaun. Translated by Professor F. W. Oliver, assisted by Misses Busk and Ewart. (Blackie.)

It is but few years since the General Biology, or Natural History, of Plants became a theme for serious investigation. Nowadays the relations between plants and their surroundings form the subject of a critical scrutiny. The older school of workers in this branch of botany were characterised by their imaginative power and credulity with regard to new theories, rather than by the exercise of any searching enquiries into the truth of their hasty hypotheses. Professor Kerner belongs distinctly to the old and romantic school. It were an exaggeration to say that he is more of a poet than of a botanist. For his book is no mere collection of light fancies; indeed, it includes a rich store of interesting facts which, for the most part, can be gleaned elsewhere only in the scattered papers of specialists. Yet the fact remains that this work, though delightfully fresh and supremely suggestive, is utterly unreliable. With an untamed dogmatism born of a hyperæsthetic imagination, Professor Kerner treats aëry conceits as proven truths. Hence many botanists may doubt the wisdom of translating such a work. Be this as it may, it is easy to appreciate the infinite difficulty involved in the labour of adequately editing an English version. Professor Oliver and his associates have evidently decided to let the work stand as it is in the original. So the theory of the centripetal and centrifugal conduction of water down plants, and the semi-fabulous account of *Lathraea squamaria*, are repeated in the translation. Even if warning footnotes be not put in connection with cases of this sort, surely erroneous, misleading, or heterodox statements should not be left unchallenged. Yet on page 64 we find it indicated that the free nitrogen of the air is not available to the plant save through the assistance of electrical discharges. On page 42, of chlorophyll bodies we are told that "they are produced generally in great numbers in special sac-like excavations in its body, but nowhere except where they are necessary . . ." It would have involved little labour to have given footnotes representing the generally accepted modern views on the subject; and the book could have thus been rendered a less dangerous guide.

But it may seem ungrateful to grumble at the translators' decision to leave the work unaltered, when that which they have attempted—the translation—is such a triumphant success. Professor Kerner's charming German idiom is reflected in clear and graphic English. In its form and get up the English version surpasses the original; in fact it is hard to see how the work could in any way have been improved as a translation. Doubtless this eager and persuasive book will do much towards popularising the study of botany in England.

Percy Groom.

Magazine Articles.

Index to the Periodical Literature of the World (covering the year 1893). [Compiled under the superintendence of Miss E. Hetherington.] 4to. Pp. 224. London: *Review of Reviews* Office, 1894. Price 5s.

Mr. Stead's generous effort to give readers a cheap and ready book of reference to the countless articles appearing in ephemeral literature is a marked improvement on its predecessors. Many suggestions as to additions and revisions have been made, and the majority of them have been accepted. The index proper has grown to 150 pages, 40

pages more than in the volume for 1892, and numerous publications, both British and foreign, have been added to the lists over and above those given and indexed last year. The price of the book is absurdly low in comparison to its value, and there can be no doubt that at present at all events its publication is undertaken at a considerable loss. As before, Miss E. Hetherington and her able staff of women assistants are responsible for the compilation of the index, and the result is worthy of the highest praise.

The first 58 pages are devoted to a list of magazines and other periodicals arranged under countries in four columns, giving the title of the book, the editor, publisher, and explanatory remarks. The remaining 150 pages give a minute and detailed index to the subjects contained in the publications listed. To turn to one entry in this index—Birds—we find no less than 55 articles dealing with the subject were published in magazines during 1893. It may be quite true to say that these are of no particular value and might well be left to perish unrecorded; but, on the other hand, we have the inestimable boon of being able to find out if such and such an article has been written without the wearisome task of searching high and low for it. Turning to another subject quite outside our own, we find under Musicians a record of no less than 100 names of which are given particulars of more or less interest.

We cannot of course enforce the purchase of this book, much as we should like to do so, but we hope few readers of NATURAL SCIENCE will refuse to place upon their shelves a volume so valuable to those who wish to keep themselves in touch with general subjects. With an increased list of subscribers, moreover, comes a corresponding increase in usefulness, for a return of the expenditure will allow of the inclusion of periodicals at present omitted solely for reasons of expense.

THE COUNTRY MONTH BY MONTH.

SOMEWHAT late in the day a copy has been sent us of F. A. Knight's "By Moorland and Sea" (Elliot Stock, 5s.); a re-issue of articles that have appeared in various publications. Mr. Knight is pre-eminently an out-door naturalist, and his bright and brief little chapters, some of them perfect prose poems, are redolent of the fresh air, and are a delightful change after wading through some particularly dry piece of laboratory investigation. Gossipy writings of this class do not, as a rule, come within our province at all; but one chapter, "The Birds'-Nester," is a genuine piece of field natural history, and reveals the true naturalist. It is evident in every line that the writer is a careful observer in close touch with Nature, who feels to the full the truth (as he expresses it), "Happy the man in whose life some room is found to watch the happier children of the air." Some of the process illustrations, too, are strikingly good.

ANOTHER work of the same class is now being issued in monthly parts by Bliss, Sands & Foster, under the title of "The Country Month by Month." It is written not for regular collectors or professed naturalists, but for the crowd of intelligent "general readers" who are now tempted into the country by cheap excursion trains or the cycle. We have received the parts for April and May. Each consists of a short essay on the month, followed by chapters on the plants and animals that may be seen in it. The essay is composed in the main of a series of quotations from poets, good, bad, and

indifferent; the chapters on the plants give a pleasantly worded account of the most interesting flowers that are likely to attract attention. A note on "Wild Life: Furred and Feathered" introduces us to the birds and beasts. An appendix gives a list of the species referred to, with their scientific names. The natural history is written in the sketchy style of Richard Jefferies and the "Son of the Marshes," the latter of whom is the senior editor. Prof. Boulger is responsible for the botanical section of the work. The parts are issued in paper or cloth at one or two shillings respectively. We recommend them heartily to those who are fond of country rambles and wish for help in knowing where, when, and how to look for our most interesting plants and animals. We fear, however, the unsophisticated townsman is not likely to see everything mentioned unless he is ready to rise a good deal earlier than most men do taking holiday in the country.

New Serials.

We have to welcome a new scientific publication from Portugal, the *Annaes de Sciencias Naturaes*, published by Augusto Nobre (8vo. Porto, no. 1, Jan., no. 2, April, 1894). Number one contains, among other matter, a list of plants of the Porto district, by Edwin Johnson; observations on the nervous system of *Arion lusitanicus*, by Augusto Nobre; the birds of Portugal, by W. C. Tait; notes on a large specimen of *Orthagoriscus mola* (the moon-fish), by A. Girard; and numerous notes on matters of local general interest, of which may be mentioned Pisciculture in Portugal, *Narcissus cyclamineus*, and the zoological station of Cascaes. The number is illustrated by three excellent photographic plates from the living plants, showing *Oxalis purpurea*, *Senecio scandens*, and *Narcissus cyclamineus*; and by a plate of the nervous system of the *Arion*, described by Senhor Nobre.

Number two contains papers by Dr. Lopes Vieira on the fresh-water fishes of Portugal, and upon the habits of *Petromyzon*; continuations of Johnson's, Nobre's, and Tait's papers referred to above, and a note on the molluscan fauna of the islands of St. Thomas and Madeira, with a plate. Illustrations of *Petromyzon fluviatilis* and *P. marinus* are also given.

We hope that this journal, so creditably got up, will meet with the success it deserves. It is to be issued three times a year, the annual subscription being 1 m. 800 (=8s.). Though Portuguese is the language of the editors, we are glad to observe that other languages are not excluded, as witness several contributions in French.

Another new serial reaches us, from Trinidad, no. 1 of the *Proceedings of the Victoria Institute of Trinidad*, an institute which was founded as a memorial of the completion of fifty years' reign of the Queen. In the articles of association we do not see any evidence of the religious prejudice which characterises an institution bearing a similar name in this country. Three papers are printed, viz., Guppy on "Edible Molluska of Trinidad"; Hart on "Ventilation"; Nabe on "Butterflies." They are more educational than scientific. The *Proceedings* are published at "The Mirror" Office, Port-of-Spain.

OBITUARY.

GEORGE JOHN ROMANES.

Born May 26, 1848. Died June, 1894.

PROFESSOR ROMANES was of Scotch extraction, and was born in Canada in 1848. He was not a public schoolman, but was educated at a succession of private schools, and by tutors in London and abroad. He had a distinguished career at Cambridge, and being possessed of private means, was able to devote himself to research after his University career. This he did with so great success that in 1873 he was Burney Prize Essayist, in 1875 Croonian Lecturer to the Royal Society, and in 1879 he attained to the fellowship of the Royal Society. He was well known as a lecturer at the Royal Institution, at Oxford, and at Edinburgh, but most people came to know his name by his abundant contributions to periodical literature. His many personal friends speak in the warmest manner of his personal qualities, and everyone interested in biology regrets his premature death. We do not profess to write with any great private knowledge of him; but we would call attention to the handsome tribute paid to his memory in the columns of *Nature* by Professor Lankester, who was at once his friend and his controversial opponent.

Professor Romanes was a fertile and ingenious exponent of the Darwinian theory, a critical and profound reasoner on biological philosophy, and a brilliant investigator. His exposition of the orthodox Darwinian theory, published under the title of "The Scientific Evidences of Organic Evolution" in Macmillan's Nature Series, is by far the most lucid, orderly, and simple publication on the subject. The first volume of his "Darwin and after Darwin" (Longmans, Green & Co., 1892) is a more detailed but equally intelligible account of the same subject. In the multitude of books written about or round about Darwinism these two writings of Professor Romanes stand out by their correctness in detail, by their literary merit, and luminous simplicity.

His most notable original investigations were upon the nervous systems of Star-fish and Medusæ. These, for the most part, were contributed to the Royal Society's *Transactions*, but afterwards a popular *resumé* of them was published as the volume on Jelly-fish, Star-fish, and Sea-urchins in the International Scientific Series. The minutiæ of structure and histology did not attract his attention so much as

the study of function; and he investigated the anatomy of these organisms much less than their physiology. Although his work on them was not sufficiently minute or accurate to rank him among morphological specialists, it led him to the equally important study of mental phenomena in animals. Darwin left to him many unpublished notes on Animal Psychology, and upon these, as well as upon his own investigations, were founded the volume upon "Mental Evolution in Animals," and a later volume on the "Origin of Human Faculty." These books have been the occasion of much controversy, but unquestionably they are greater contributions to Comparative Psychology, and hence to the study of the mind of man, than most psychologists were ready to admit. We prophesy that, as now Psychology has abandoned its concubinage with Metaphysic for a fertile union with Physiology, Professor Romanes's work will receive a place more important than that presently assigned to it.

George John Romanes.

His contributions to Biological Theory are perhaps the least important part of his works. His theory of physiological segregation was ingenious, but it was based on far too little observation to bear the superstructure he raised on it. In his controversies in various periodicals, he was more apt at arguing over verbal points, and laying stress on "logical inconsistencies," than at appreciating the facts which gave origin to the theories in question. We have had occasion in Natural Science to point out in several instances what we thought defects in his arguments. But it is just and pleasant to add that he was very ready to modify his own views or his conception of the views of others when the stress of facts lay against him, and that this logical acumen served often to expose concealed weaknesses in the arguments of others.

In later years Professor Romanes was conducting a variety of

experiments on an extensive scale, that, had they not been stopped by his untimely death, would have been of great importance in settling many obscure questions of inheritance. Certainly English science is the poorer for the death of this worker, who gave so much time, talent, and ability to the investigation of its more abstruse problems.

We are indebted to the courtesy of the Editor of the *Illustrated London News* for the portrait here given.

Dr. August von Klipstein, whose death, on the 16th of April in this year, we recorded in our last number, was the son of the mineralogist, Philipp Engel von Klipstein, of Darmstadt, and was born on June 7, 1801, at Hohen-Solms, near Giessen. In 1836 he was appointed Professor of Mineralogy at Giessen. He is perhaps best known by his monograph on the geology and palæontology of the Eastern Alps, but has also made valuable communications on the geology of the Kupferschiefergebirge, the Odenwald, the Dukedom of Hesse, Upper Suabia, and the Vogelsgebirge. In 1837 he described, with J. J. Kaup, the *Dinotherium giganteum* from Darmstadt.

Dr. Jozsef Szabó de St. Miklós, who, as already announced, died at Budapest on April 10, at the age of 73, was well-known for his studies in mineralogy, of which science he held the chair in the University of Budapest. He was also President of the Hungarian Geological Society, and secretary to the Natural Science section of the Hungarian Academy. Dr. Szabó was an enthusiastic collector, and the University Museum owes much to his energy and exertions.

Dr. K. A. Fiedler, Privatdocent at the University of Zürich, died on April 2, at the early age of 31. Dr. Fiedler had published, among other things, some interesting experiments on the development of Echinoderm eggs similar to the experiments of Driesch, Herbst, and others.

We have also to record the deaths of the algologist, Alphonse Derbès, Honorary Professor at the Faculty of Science at Marseilles; of Dr. Thomas Morong, Curator of Columbia College, who died on Thursday, April 26. Dr. Morong was especially known for his work on Potamogetons and other allied water plants; he also collected somewhat extensively in Paraguay. And of Knut Fredrik Thedenius, at Stockholm, on March 4, aged 80. An apothecary and a teacher of Natural History in the Stockholm Gymnasium, Thedenius was the author of several works on Swedish local floras, and also, in conjunction with N. J. Andersson, of a "Svensk Skol-Botanik," containing 250 coloured plates and descriptions of Swedish plants.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Important changes have recently been made in high scientific posts of the United States Government. Major Powell, who for thirteen years has been head of the Geological Survey, has resigned on account of ill-health, due to enlargement of the nerves in his wounded arm. Major Powell is an ardent explorer and ethnologist, and it has been largely through his efforts that there is now so complete an understanding of the Californian Gold Belt and the Rocky Mountain region. Major Powell will not, however, retire into private life, but will become superintendent of the Ethnological Bureau, which was recently severed from the Geological Survey. His successor, as head of the Survey, is Mr. Charles Doolittle Walcott, whose nomination will be heartily welcomed in this country by all who appreciate his thorough and painstaking investigations in the palæontology and stratigraphy of the older rocks of America. There has of late years been some exception taken in certain quarters to the lines on which the work of the Survey has proceeded, but we understand that the present change has nothing whatever to do with these attacks, and that the labours initiated by Major Powell, especially the great topographical map of the United States, will be continued without alteration.

Professor C. V. Riley, for twenty years chief entomologist of the Department of Agriculture, has resigned his post in order to devote himself to scientific work unfettered by the restrictions that necessarily surround a Government servant. His retirement will be regretted by both practical and scientific men, whom the work done in his bureau has so greatly benefited. He is succeeded, at his own recommendation, by Mr. Leland O. Howard, who has been first assistant in the bureau for fourteen years, co-editor of the *Bulletin* with Professor Riley, and is now President of the International Association of Economic Entomologists.

Professor Milton Whitney has been placed in charge of a newly created Division of Agricultural Soils, in the United States Department of Agriculture, which Division is intended to study the relation of soils to crops and the physics of soils.

Lastly, Professor W. H. Holmes has severed his connection with the Bureau of Ethnology to accept the position of head of the department of anthropology in the Columbian Museum at Chicago.

Among recent European appointments we note those of Dr. S. J. Hickson to the Professorship of Zoology in the Owens College, Manchester; Professor E. Kalkowsky of Jena to the chair of Geology in the Dresden Technical School, and not Dr. Credner as previously announced; Dr. L. Will to the Professorship of Zoology in the University of Rostock; and Forstassessor G. Sarauw to be assistant in the National Museum, Copenhagen. Professor R. Semon has resigned his position as assistant at the Anatomical Institute of Jena, and Doctors Braus and Drüner have been appointed in his place. We take this opportunity of congratulating Dr. Pfitzer, the Professor of Botany at Heidelberg, on his appointment to the dignity of Geheim-Hofrath, and the University of Oxford on the fact that Francis Galton is now one of its Doctors.

The following awards have recently been made:—The silver medal of the Zoological Society of London to Mr. H. H. Johnston, C.B., in acknowledgment of

the efforts he has made to increase our knowledge of the zoology of British Central Africa; the gold medal of the Linnean Society of London to Professor E. Haeckel, who was unable to be present to receive the honour on account of recent illness; the Demazières prize of 1,000 francs, in the award of the Paris Academy of Sciences, to the botanist, M. Sauvageau, while the Montagne prize, of the same value, was divided between two other botanists, MM. Cardot and Gaillard.

At the Ladies' night of the Royal Society, on June 13, Professor McKenny Hughes had an exceedingly interesting exhibit of ancient horns, illustrating the evolution of the breeds of English oxen. In the Palæolithic age there were three species—*Bison priscus*, *Bos primigenius*, and *B. longifrons*. Only the latter survived till the arrival of the Romans, by whom it was crossed with a larger imported breed, having straighter and more upturned horns—a type which is still seen in the tawny Highland cattle and in the Chillingham cattle. All these are whole-coloured; the parti-coloured cattle are a much later introduction. After the withdrawal of the legionaries, stock soon ceased to be selected, and the cattle reverted to the type of *Bos longifrons*.

The Marine Biological Association had added to their exhibit a sole from the North Sea, in which the eye had not shifted from the left to the right side of the body, so that the head remained undistorted. A similar case was quoted last year in a French scientific journal, but the occurrence is a rare one.

Mr. Hutchinson, whose last book we notice in this number, had some small papier maché models of *Megatherium*, *Dinoceras*, and *Plesiosaurus*, which would have pleased us better had some texture been imparted to their surface. Calvity is the prerogative of the most highly-developed among us.

Professor Stewart, with the aid of three specimens of the common shore-crab, taught us how to walk sideways, and showed what modification of locomotory appendages takes place in the hermit crab.

Miss Edna Walter, B.Sc., is, if we mistake not, the first lady that has exhibited at the Royal, and we must congratulate her not only on her exhibit, but on the very lucid explanation she gave of it. It was a goniometer, devised by herself and Mr. H. B. Bourne, for the purpose of projecting a crystal on a sphere, and its value is rather to demonstrate the fundamental axioms of crystallography than to assist in the practical measurement of crystals. Miss Walter has, we understand, further added to the triumphs of her sex by exhibiting this same apparatus at the last meeting of the Mineralogical Society and lecturing upon it *in propria persona*.

The conversazione given by the Oxford University Junior Scientific Club on May 22 was, in spite of the weather, one of the most successful that we remember, and was attended by over 800 guests. The exhibit that attracted most attention was a block of solid carbon dioxide, weighing 40 lb. This had been sent from the brewery of Messrs. Guinness, in Dublin, packed in a barrel with wool, and had only lost 10 lb. on the journey. The temperature of the solid substance is $-76°$ C., and by its means mercury was solidified and shown to be malleable, while ether was rolled into candles and burnt. Among exhibits of more direct interest to ourselves were a collection showing the adaptation of plants to environment, particularly that of deserts, and experiments showing the distribution of seeds, movement of tendrils, and formation of starch; living marine animals, amœbæ, rotifers, and the like; interesting histological preparations from the human subject; while, despite the Anti-vivisection Society, several physiological experiments were performed on not unwilling visitors, many even shedding their blood in the cause of science. During the evening Captain Lugard gave a very interesting account of East Africa, which evoked the political enthusiasm of the audience.

At a recent meeting of this Society, Mr. A. Vassall read a paper on the Recapitulation Theory, in which he alluded to Dr. Hurst's paper published in our pages. It is, we take the liberty of telling Mr. Vassall, by no means fair to say that "Hurst's opposition simply consists of a direct negative."

In the final school of Animal Morphology at Oxford, Miss Lilian J. Gould, of

Somerville Hall, and Mr. M. D. Hill, of New College, have been placed in the First Class. In the current number of the *Quarterly Journal of Microscopical Science*, there is a paper by Miss Gould on the structure of the fresh-water Rhizopod, *Pelomyxa palustris*, to which Mr. Hill contributes a note. Both have also contributed to our own pages.

THE Conchological Society of Great Britain and Ireland held its annual meeting in Manchester, on June 9, when the President, Mr. W. E. Hoyle, delivered an address on the Classification of the Pelecypoda (Lamellibranchiata).

ON page 315 of our last volume we announced that Miss Brocklehurst had offered some £2,000 to erect a Museum in Macclesfield Public Park. Plans had been prepared for her by a well known architect, embodying the advice of museum experts. Unfortunately, these plans did not meet with the approval of the local surveyor. But the Town Council have now learnt that it is unwise to look a gift horse in the mouth. Miss Brocklehurst has withdrawn her offer.

THE University College of North Wales, Bangor, will, in October, open a Day Training College for the instruction of sixty students equally divided between the two sexes.

A SCIENTIFIC exploring expedition will shortly start for the Macdonnell Ranges in Central Australia. The expedition is at the cost of Mr. W. A. Horn, a member of the House of Assembly of South Australia. All the Australian Colonies, however, have been invited to send scientific men, and we learn that Professor Baldwin Spencer, of Melbourne, intends to take this opportunity of studying the fauna of this little-known region.

MR. W. H. SHRUBSOLE, F.G.S., of Sheerness-on-Sea, has been good enough to send us a paper announcing that he is prepared to accept engagements to deliver lectures on the "Curious Dwellers on our Shores," also on "Coal and its Products." It is not our custom to give advertisements for nothing, but, in the intervals of a busy life, Mr. Shrubsole has done so much for Natural Science that NATURAL SCIENCE can hardly refuse so slight a return.

WE learn from the *Botanical Gazette* that the Botanists of the United States are combining for the purpose of making "a compact and complete presentation of the North American flora, so far as it is known." Arrangements have been made for the publication of a "Systematic Botany of North America," under the editorial control of eight well-known botanists, while the co-operation of at least thirty more has already been secured. The work is to appear in seventeen volumes, with about five parts to each volume, and 100 pages to each part. It is hoped that five or six parts will appear annually, beginning with 1895. There will be no illustrations, but full references will be made to published figures. "Special features will be the examination of type-specimens, the citation of type localities, geographical distribution, and a discussion of the economic, palæontologic, and horticultural features of each order." As regards sequence of orders, the editors propose to follow Engler and Prantl in the "Pflanzenfamilien." It is thought that such a work will make information so accessible that study will be stimulated and knowledge extended much more rapidly. Its course of preparation also will doubtless develop many new investigators. The *Gazette* does not state what are to be the geographical limits of the flora, whether, for instance, "North America" includes Canada, which, we notice, is not represented on the editorial board.

A SECOND edition of the concise Guide to the Mitchell Library, Glasgow, has recently been issued. This free public library was provided by Mr. Stephen

Mitchell, a tobacco manufacturer of the city, who died in 1874. It was opened in 1877, in temporary premises in Ingram Street, but since 1890 has occupied the buildings in Miller Street formerly used by the Water Commissioners. Some idea of the size of the Library may be obtained from the fact that no less than 349 magazines and periodicals are placed during currency on the tables in the Magazine Room. Among them Natural Science occupies a prominent position, such journals as the *American Naturalist*, *Annals and Magazine of Natural History*, *Botanical Magazine*, *Entomologist*, *Entomologists' Monthly Magazine*, *Geographical Journal*, *Geological Magazine*, *Quarterly Journal of the Geological Society*, *Journal of the Royal Microscopical Society*, *Mineralogical Magazine*, NATURAL SCIENCE, *Nature*, *Science*, and *Zoologist*, finding a place in the series. One of the excellent rules provided by the representatives of the Founder sets forth that the Library should aim at representing every phase of human thought and every variety of human opinion, and that no book should be refused admission merely because it controverted present views on religion or politics. After all, Glasgow is not so black as she has been painted.

AN International Congress of Applied Chemistry will be held at Brussels, from the 4th to the 11th of August, under the patronage of the Belgian Government. There are four sections, representing (*a*) the sugar industry, (*b*) agricultural chemistry, (*c*) alimentation and public hygiene, (*d*) biological chemistry, dealing chiefly with fermentation. Among the objects to be undertaken by this Congress, we are very glad to note "un ensembles de mesures destinees a faciliter aux specialistes et techniciens l'acces rapide de toutes les publications qui les interessent." Those who wish for further details of the Congress should apply to Mr. F. Sachs, Rue d'Allemagne, 68, Bruxelles.

THE Shah of Persia has instituted a zoological garden mainly for the purpose of introducing the native animals of Europe to his subjects.

THE Vienna Academy of Sciences are preparing an expedition, under the direction of Dr. Steindachner, to explore the deep waters of the Mediterranean.

CORRESPONDENCE.

The Natural History of the Flower.

Mr. J. C. Willis (Natural Science, vol. iv., p. 347), I am glad to see, has arrived at precisely the same result in his experimental study of flowers as I maintained six years ago in my book, "The Origin of Floral Structures," etc., viz., that the phenomena bearing on sex, cleistogamy, etc., "are primarily due to the action upon the plant of external causes." The only point to which I cannot follow him is when he says, "Natural Selection apparently only begins to act later on." I would ask what *facts* are producible to *prove* that Natural Selection acts at all on the maintenance, if not the origin, of any floral and, indeed, other structures? Be it observed that his first conclusion, just stated, is based on positive facts which are accumulative; the second, that Natural Selection plays a part, is an *assumption* for which he supplies none.

Mr. Willis is by no means alone in thus introducing Natural Selection. For example, Mr. Cockerell, in describing the dwarf character of many high Alpine plants, which he attributes to the direct action of the environment, adds: "It appears, however, that Natural Selection may come into play," etc., and gives three hypothetical applications (*Nature*, xliii., 1891, p. 207). It seems to me these and other similar appeals to Natural Selection are simply a sort of unconscious tribute to Mr. Darwin; but since the adaptations as described by these authors are all illustrations of plants varying in *definite* directions in response to the environment, surely it would be a higher tribute to him to quote his own description not only of such an origin of variation, but of its maintenance. For not only does he say that "Natural Selection has no relation whatever to the primary cause of any modification of structure" (*Anim. and Pl. under Dom.*, ii., p. 272); but he observes: "By the term definite action I mean an action of such a nature that when many individuals of the same variety are exposed during several generations to any change in their physical conditions of life, all, or nearly all, the individuals are modified in the same manner. A new subvariety would thus be produced without the aid of Natural Selection." (*Anim. and Pl.*, etc., ii., p. 271).

Mr. Darwin, arguing from cultivated plants—in which *indefinite* variation is the rule, so that artificial selection is absolutely necessary—*imagined* indefinite variation to be also the rule in Nature, and *definite* variation to be the exception. Such is, however, not the case. Variation in Nature is always, as Mr. Willis, Mr. Cockerell, and many other writers have of late years maintained, in strict adaptation to the direct action of the environment; in other words, *natural variation is always definite*. Hence Mr. Darwin himself proves, though contrary to the title of his book, that the origin and maintenance of specific characters are without the aid of Natural Selection.

George Henslow.

Sir,—From the fact that our flora resembles the flora of northern Europe in being visited by a larger proportion of flies than has been observed in the neighbouring regions of the Continent, Mr. J. C. Willis infers that "our flora would seem rather more closely allied to these northern floras in this respect than to the nearer Continental flora" (Natural Science, iv., p. 349). Before this inference can be accepted it will be necessary to prove that this resemblance is not merely the result of similar circumstances, such as the natural preponderance of flies over other insects in our own and northern countries.

It may also be pointed out that Mr. Willis's general conclusion, at the bottom of p. 350, implies the inheritance of acquired characters. External causes, he says, produce various degrees of sexual development, on which "Natural Selection only begins to act later on." But Natural Selection, we are told, can only act on characters that are congenital. Does Mr. Willis appreciate the importance of his own conclusion, and what has he to say to this interpretation of it?

Entomophilos.

The Hatching of the Octopus.

Miss Agnes Crane writes expressing astonishment at the ignorance displayed in our notes on the octopus in last month's "Notes and Comments." She says:—

"As a matter of fact the 'brooding' of the 'hen' octopus and the hatching of the eggs was a fifty days' wonder at the Brighton Aquarium many years ago, and the subject of general comment in the daily and weekly Press. I have watched the female often hovering over the two dozen or more live oysters she had sedulously accumulated in a rocky crevice of her tank near the glass for weeks together. Full descriptions appeared in the *Times*, *Land and Water*, *The Field*, etc., at the time, and Mr. Henry Lee subsequently devoted a whole chapter to 'The Spawning of the Octopus,' recording his observations in his 'Aquarium Notes' on 'The Octopus of Fiction and the Octopus of Fact,' published by Chapman and Hall in 1875.

"Therein he relates that the female octopus deposits her eggs, 'which look like little grains of rice,' in strings, at intervals, for three days, a fair-sized specimen producing ten or twelve long strings, or clusters, or a possible progeny of from forty to fifty thousand individuals, that she broods over them, guards them, and cleanses them, taking but *little food* the while, and becoming quite exhausted by her maternal cares.

"Moreover, your contributor used the word 'incubation' loosely, and seemed to imply that the octopus sits upon its eggs for the purpose of hatching them by the heat of her body. Mr. Lee held this theory not proven, as he succeeded in hatching out a brood of several hundred young octopods from ova which had been removed from the mother in question and placed in a tank by themselves.

"Young octopods are described 'as almost as big as a flea, and, when irritated, of much the same colour,' and as able to assimilate their hue to the colour of their surroundings even before they have left the envelope. The brood referred to lived about three weeks, but ultimately succumbed, possibly to the over-assiduous attentions of their scientific dry nurses: at that time Mr. Frank Buckland, Mr. Henry Lee, and Mr. W. Saville Kent were all professionally attached to the Institution. The eggs of the Sepia and of *Loligo* were also hatched out independently at the Aquarium when removed from parental care. The octopus, therefore, probably guards, cares for, and hatches her eggs just as the nest-building 'fifteen-spined' sticklebacks do, and not in the usual sense of the word incubation. Mr. Lee stated 'that octopods he had known' differed much in their manner of 'brooding' over their eggs. He also recorded the occurrence of a full-grown female octopus and her eggs within an earthen two-gallon carboy which had been dredged in the channel, and of which the neck measured only two inches in diameter. But I have said more than enough to prove that the 'hatching of the octopus' has not been forgotten since the days of Aristotle.

[We protest against the assumption that "incubation" involves heat. The term actually was applied to fish by Owen.—Ed.]

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NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 30. Vol. V. AUGUST, 1894.

NOTES AND COMMENTS.

Vox Populi.

WHATEVER Mr. Arthur J. Balfour says is interesting, whether one agrees with it or not, and most people will therefore have read the remarks that he made when he took the chair at a recent lecture by Professor J. Shield Nicholson. One of Mr. Balfour's points was the necessity for a separation between science and popular opinion. Questions of science cannot be decided by universal suffrage. Science must be allowed free play and room for growth altogether outside the influence of popular forces. Science must be kept from the vulgar, and the vulgar must be kept from science.

Looking at things, as is his wont, through the clear and rarefied air of philosophy, Mr. Balfour could not fail to apprehend the essential distinction that must always exist between the intellectuality of scientific investigation and the sentiment of popular feeling; he could not fail to discriminate between the attitude of mind that pursues knowledge for its own sake, sacrificing the most cherished convictions of the multitude on the altar of a severe truth, and that very opposite spirit which views all things through the distorting glasses of utilitarianism, seeking always for some advantage either in this world or in that which is to come. Now these two modes of approaching Nature are so distinct that it is quite impossible to unite them. Each is bound to live and move independent of the other. So far, then, as Science is concerned, we do not think that the public will ever spoil her investigations. Mr. Balfour's warning is therefore not required.

When, however, we look at things from a somewhat more practical standpoint, we shall discover many advantages that may accrue to both parties from the promotion of a closer alliance between the two than Mr. Balfour seems to favour. Not merely need we have no fear of injuring Science, but we may even gain for her some additional powers. Scientific men have, or ought to have, a wise diffidence in the stability of their own theories; perceiving how far

the unknown still exceeds the known, they would be the last to rashly alter their conduct in obedience to each variation of belief that appears to be necessitated by each fresh discovery. But they are unable, even if they wished, to keep the new discoveries and the new theories from affecting the larger public. Here a danger lies. This public, vaguely expectant of some message or some new rule of conduct, eagerly grasps at such fragments of scientific theory as are conveyed to it in the diluted form of books like "The Ascent of Man." Then, of a sudden, the vessel of science tacks, and the public, seeing itself left behind, cries out that it is deceived and turns its former praise into ridicule or contempt. Our hope of progress then would seem to lie, not in refusing the laity such crumbs of scientific fact as they may have a fancy for, but in giving them to understand what is the true scientific spirit.

Often enough in these pages we have dwelt upon the aid that can be given to science, not merely by the scientific amateur, but by every member of the public. Intimate relations are bound to exist between the two sides, and instead of withdrawing ourselves into some cloistered calm, we desire to promote those relations that they may enter the phase of a cordial and intelligent affection. Still a warning is needed. It is by no means desirable that those without a proper training should attempt to engage in the more technical branches of scientific work. No one has any objection to people amusing themselves with music or painting, but we do not wish to have our ear distracted by immovable piano-organs or our eye affronted by glaring crudities of advertisement. So with scientific work. There is too much done by well-meaning people that has merely to be undone by the despairing specialist. What we must aim at is to direct the praiseworthy energy of these good folk into channels of more service to us and of no less profit to themselves.

The British Association.

In extending the brotherhood of Science, not merely among the workers themselves, but also, as just intimated, with the world at large, one of the more important instruments is the British Association. For this Association, unlike some others which have been founded in imitation of it, has never forgotten that one of its chief aims is to introduce the facts and principles of Science to those who would otherwise pass them by as belonging to a sphere other than their own. And this idea is really the explanation of much that some of the more ascetic enthusiasts occasionally object to in the working of this organisation. The popular lectures, the social gatherings, and even the picnics play their own good part in the production of a better understanding between the usually indifferent public and those whom they welcome as dreary pedants, but part from as good fellows like themselves.

At the least it is admitted, even by the grumblers among us, that this annual gathering extracts for Science an amount of nutriment in the shape of golden guineas not altogether to be despised. But what we would rather lay stress upon are such benefits as the fresh impetus given to scientific investigation in each place that is visited, the friendships that professional workers often form with local amateurs, and the general awakening of the public to the fact that, if on the one hand Science helps them, it is, on the other hand, well within their power to offer in return help in many ways previously unthought of. Looking at the matter from these two points of view, there are certain considerations that we may take this opportunity of bringing to the notice of our readers, many of whom will doubtless shortly be attending the meeting at Oxford.

Firstly, as regards the matter brought to the notice of the Sections. It may not be too late to express a hope that contributors of papers to the forthcoming meeting will remember that there is a large body of members who expect entertainment other than the customary highly complicated paper dealing with minute details in an obscure corner of scientific work. Many people bring dry papers which would rapidly empty a learned society, and void them on a respectable hard-working audience which has come to hear some new thing that is addressed to their intelligence. The aspect of the audience, if it remain under such an infliction, is generally saint-like and resigned; in fact, a British Association audience is one of the very best in the world, and all rightly thinking and feeling people hate to see it tortured. One of the things most appreciated in the past was the formal discussion of some selected subject. This was brought into contempt, principally, if our memory serves, by a discussion on stays some years ago; and while it might be advisable to refrain from inviting a possible recurrence of such demoralisation, something ought to be found to take the place of the "discussion." The secretaries of Sections have an opportunity here of improving the meetings by bringing together the papers on allied subjects, having them read without comment at the time, and finishing up each group of papers with a general discussion of them. It would save time, give wider scope to discussion, and make things more interesting to the outsider and probably to the scientific Pharisee as well. A time limit should be fixed for each group of papers, and the discussion of them could be announced in the daily journal; thus members would learn not only where they were, but where they wished to be at given times.

Turning now to the finance of the Association, we hope that someone will raise the question of the invested funds at the general committee. A representation as to this matter was recently made to the Council by a body of scientific men of exceptional weight, and it was pointed out that the invested funds greatly exceeded the necessary amount set aside against liabilities to life-members. It was asked

that the balance should be used in the lean years of the Association to supplement the grants-fund. The present position of affairs is unbusinesslike and deserves another description than "cautious," for which quality some praise it. Every penny, beyond the reserve fund to secure liability to life-members, should be spent. The Association has no authority to treasure it up for posterity; the money belongs to the contemporary members, who wish to see it put to useful work.

Among those to whom we have previously alluded as perplexing Science with their unwelcome attentions, a foremost place must be assigned to the British Association "crank," that peculiar type which appears annually for one crowded week of glorious activity and hibernates—no man knows where. The delicate question of dealing with these dear people has always an importance at a great meeting, such as the Oxford one will be. Many things have been tried, closure, etc., etc.; but the most successful is an enterprising, courteous, spirited, and ingenious secretary attached to a Section for this special duty. The system has its drawbacks, since, in one instance of marked success, the zealous official in question had actually to flee from the vengeance that awaited him when his wiles were discovered, and he has not dared to show his face again at a British Association meeting. But the cranks come back.

The Marine Biological Association.

Among scientific institutions that justly seek for the aid which, as we have said, the public can so well bestow, must be mentioned the above Association, which has just distributed a letter signed by the President, Professor Lankester, and the Hon. Secretary, Mr. G. H. Fowler, accompanied by a brief biography of the Society. As is the case with most of our scientific societies, the Marine Biological Association has not, by any means, too large an income for its needs. Considering that the Government recognises the importance of the investigations, carried out under the direction of the Council of the Society, in economic questions affecting our fisheries, by an annual grant of money, it is to be hoped that the public will individually reply to the appeal of the Association. The cost of membership is not large—one guinea per annum—and a considerable extension of the list of members would remove many restrictions to the usefulness of the laboratory. The researches carried out have resulted in a great deal of information with regard to such important matters as the size at which various food-fishes reach maturity, which should lead to useful legislation on the artificial fertilisation of fish, the history of the sardine, which has been shown to be simply a pilchard one year of age, and so forth. The more purely scientific work cannot be supposed to appeal to the public at large, important though it is. But those persons who appreciate the morning sole and the midnight lobster

might be expected to further the more abundant supply of these dainties. The efforts of the Association in breeding and protecting these and other food-fishes, backed by legislation against their unnecessary destruction, will unquestionably improve our fisheries; but there is a need for increased financial support—the total income being at present only £2,199, as against £70,000 which the United States Fish Commission gets from the American Government.

Natural History as a Recreation for Sailors.

We have mentioned the aid that laymen can render to Science, and few of them have better opportunities for observing natural phenomena and natural objects than they that go down to the sea in ships. And yet we have often been surprised at the small amount of collecting and observing that is done by Naval men in comparison with their brothers of the Army, although in distant parts of the world both alike have time on their hands and freedom to use it. Of course, we do not now refer to those special exploring expeditions like those of the "Challenger" and the "Blake" in recent years, or the long-sustained explorations of the French in the early part of the century, but to the private collection that is or might be done by those stationed abroad; and India in particular comes to our memory. We are therefore glad to see, in the *Nautical Magazine* for May, a short paper with the title that we have placed at the head of this note. It is written by Captain D. Wilson-Barker, who, as captain of the training-ship "Worcester," has excellent opportunities of instilling into the young mind some appreciation of the beauties and interests of marine animals and plants; and it is to be hoped that our future race of seamen will be more prolific in collectors than that of the past has been.

We are especially interested in one paragraph in this paper, and that is the following:—"It is curious to see the particular whale which frequents this coast [Brazil], and has very long pectoral fins, gambolling about, waving high in the air those long fins, which, in the distance, glisten like gigantic sword-blades." In our March number, it will be remembered, Mr. Lydekker gave an account of a whale-fight off the same coast. Captain Wilson-Barker says, in a letter he has been good enough to send us, that he had an opportunity of observing these whales for eight months while repairing cables off Brazil. He says the pectoral fins, which nearly reach twenty feet in length, are constantly mistaken for threshers; the outer surface, in common with the whole upper part of the whale is black; the inner surface, in common with the belly, is a glistening white. He therefore suggests that the distinct colouring of the inner and outer surfaces of the fin, alternately exposed during the gambolling of the whale seen by Mr. Lydekker, would give the impression of there being two distinct animals. And this interesting note seems to carry conviction.

The Worm, the Gastropod, the Coral and the Bivalve.

Strange as any fairy story is this other true tale of the sea that Mr. E. L. Bouvier tells in *Comptes Rendus* for July, 1894 (vol. cxix., pp. 96-98). Once upon a time it was supposed that certain Madrepore corals, named *Heterocyathus* and *Heteropsammia*, built their skeleton around the shell of a living shell-fish, which prolonged its naturally-coiled shell in the form of a tube through the enveloping coral; and this was the opinion of Deshayes, Milne-Edwards, and Haime. This, however, was not quite correct, as Mr. Bouvier is now able to prove from material collected at Aden by Dr. Jousseaume. The curious facts are as follow.

The polyps of *Heterocyathus* and *Heteropsammia* fix themselves, probably at the close of embryonic life, on the minute but empty shells of various species of Gastropods. As soon as each shell is chosen by a polyp, it receives a new guest in the person of a young worm called *Aspidosiphon*, one of the Gephyreans. The little worm enters the shell and rolls itself in a corresponding spiral. Then the coral outside and the worm inside grow simultaneously. On the one hand, the coral soon completely covers the shell and threatens the imprisonment of the worm; but the worm, on its part, continues to grow in the form of a loose spiral, and thus prolongs the coiled cavity of the shell through the tissues of the coral, passing to the exterior by a round opening. At the same time, the worm secretes a tube, which appears at first to be an actual continuation of the shell, but which differs from it by its less thickness, its more intimate union with the skeleton of the coral, and by the rougher appearance of its inner surface. In *Heteropsammia* this tube usually reaches the external opening; but in *Heterocyathus* it is formed more slowly and does not reach the exterior. This single external opening is, however, not enough to bring aërated water for the respiration of *Aspidosiphon*, so it sets to work, apparently by a secretion from glands in its skin, to dissolve long perforations through the substance of the coral.

The worm has a long proboscis, terminated by a crown of short tentacles, and covered with several rows of pointed hooks. It can thrust out this proboscis to catch its prey, and by sticking its hooks in the ground, can, as Dr. Jousseaume has seen, drag along itself and partner. The worm has also two stout horny shields, one of which is placed near the anus at the base of the proboscis, and, when the latter organ is drawn in, it serves to close the opening of the tube. The other shield is at the other end of the animal, and its particular use is not known. The *Aspidosiphon* that lives with *Heteropsammia* is a different species to that living with *Heterocyathus*.

But we are not at the end of the story yet. A home having been thus prepared by *Aspidosiphon*, a little bivalve mollusc called *Kellia*, which seems given to these lazy habits, comes and takes lodgings in the depressions of the tube, and boards on the food that

comes in through the respiratory perforations above mentioned. Mr. Bouvier compares the commensalism of the worm and the coral with that existing beween *Parapagurus pilosimanus* and colonies of *Epizoanthus.* Similarly, the commensalism of the bivalve and the worm may be likened to that obtaining in South Australia between *Ephippodonta* and a burrowing shrimp.

Science at a Picnic.

If we have ventured to say a good word for the much-abused garden-parties and excursions of the British Association, we have no wish that such license should be extended to other bodies which are of a somewhat similar character, but which differ in the fact that the primary objects of their meetings are work and discussion.

In our news column we give some account of the work done at the recent meeting of the Museums' Association. Next year the curators are to meet at Edinburgh, and we trust that the local committee of the northern capital will learn a few lessons from the Dublin meeting. Irish hospitality was so generous and overwhelming that it seriously detracted from what should have been the first business of the meeting, the reading and discussion of papers and practical proposals. It will scarcely be credited that, though the members spent the best part of a week in Dublin, the time allotted to the long list of papers, which included many besides those we have quoted, was only five hours. Considerable complaint was heard at the way in which both papers and discussion were, in consequence, burked; while the insertion of a day's excursion between the two days of meeting proved vexatious to those who could ill spare their time even for the serious business of the Association. We think we cannot do more good to this hard-working body than by ventilating these complaints, which gratitude to its unwearying hosts would otherwise stifle.

Very similar is the cry that comes to us from the Antiquaries in congress at Burlington House. In a paper that went straight to the point, Mr. St. John Hope protested against the elaborate lunches that waste much of the time at local archæological meetings. Clear explanations of the facts that they went to study were enough to attract an audience without the addition of gratis meals. His experience was that intelligent appreciation was usually shown by an audience composed largely of working-men, which was in marked contrast to the listless inattention and often rude indifference of folk calling themselves ladies and gentlemen. Another objection to too great an acceptance of local hospitality was that they might become a tax upon the places they desired to visit. We ourselves remember an occasion when, out of a large party, only two individuals examined an interesting geological section, which the party had travelled many miles to see, but which they were hindered from studying by the prior attractions of welcoming speeches and lunch.

The Life of Museums.

Mention of the Antiquaries in the same paragraph as the Museums' Association reminds us that the former seem strangely ignorant of the existence of the latter. At all events, the upshot of a discussion on local museums, which they carried on the other day, was the appointment of a committee to investigate the questions of arrangement and financial support and the working of the Museum and Free Library rate. It is an admirable thing that the archæologists should treat provincial museums with a proper seriousness, but all these questions have been discussed over and over again by the Museums' Association; and if these gentlemen are really so interested in the subject, how is it that none of those who spoke at the congress or who have been elected on its sub-committee have, so far as we can gather, with the exception of Mr. Ward of Cardiff, ever attended the meetings of that Association?

A propos of local museums, we have received a copy of the *Yorkshire Weekly Post* for June 23, in which the Science Editor, Mr. G. W. Murdoch, has some very sensible remarks on the better utilisation of those institutions. After alluding to the usual difficulties with which provincial (and we may add other) museums have to contend, such as too great enthusiasm for one section to the neglect of others, the lack of local financial support, government by illiterate town councillors, the vanity of donors, and the utilisation of the museum as a convenient rubbish heap, he proceeds to enlarge with much approval on several remarks of our own. From his own examination of many museums, both at home and abroad, Mr. Murdoch has, we are glad to find, come to the conclusion that a museum is of greatest value for the cause of Science, and best fulfils the purposes of its being when it is intimately connected with active teaching and original investigation. The account, which we are fortunately enabled to publish in this number, of the University Museum at Oxford will show that a museum which thus co-operates with the laboratory need never fear comparison with museums that confine their functions to those of the store-room and the holiday entertainment. The ideal museum would, in our opinion, be the intermediary between its own collectors in the field and its own researchers in the laboratory, with its exhibition galleries daily open to neighbouring students, and its reserve collections always ready for the inspection of specialists from every part of the world.

Winchester Once More.

From a leading article in the last number of the *Wykehamist* we learn that the new Museum at Winchester College, to which, by the way, Mr. Murdoch alludes, is to be worked on such lines as we here advocate. "We hear talk," says the school journal, "of rooms for photography, modelling, drawing, and practical natural history, quite

apart from the exhibition rooms. It is to be a home not only of art, but of artists; not only of 'bugs,' but of bug-hunters; not only of fossils, but of those who are qualifying for fossilhood." But will so admirable an ideal, which would render this Museum a true temple of the Muses, ever be attained? We sadly doubt. We leave Wykehamists themselves to settle the question of site, as to which they have already begun to quarrel; but we would fain once more express our surprise at the scanty support this memorial of their great founder has met with. William of Wykeham was a scholar and an ecclesiastic, but he was, above all things, a practical man; we can, therefore, imagine no more fitting way of carrying on the very spirit of Wykeham into the centuries to come than by the introduction of this practical intercourse with nature and art into the educational methods of a school that has adhered somewhat too closely to the scholastic traditions of the Middle Ages. £4,319 16s. 2d. is not a large sum for such an object—is, indeed, miserably inadequate; but what is worse is that it has been subscribed by only 402 people, a number barely equal to that of the boys at present in the school. In the interests of Winchester itself, no less than those of modern education, we trust that a few more Wykehamists will come out of the holes into which they have, like the traditional college spider, for the time retired.

Science and Nature.

In our opening paragraph we spoke of the relation that daily grows more intimate between the scientific specialist in his laboratory and the ordinary man in the street or in the field. Here is an example appropriate to the season. For these are the days when tired and thirsty mortals crave above all things the refreshing charms of fruity acids. Were this a Saturday column of the *Pall Mall Gazette*, we would seek the choice word and the apt phrase to do worship before the dear delights of "lemon squash" or those more humble, prepared drinks that owe their flavour and their fragrance to commercial citric acid. An article in the *Kew Bulletin*, published some months ago (1894, pp. 103-108), spread dire consternation among those who grow the lime and the lemon in the south of Europe and in the West Indies; for it related a direct method of producing citric acid from sugar solution by growing therein a peculiar mould. But by the July number of the same periodical the fears of fruit-growers are, for the time, set at rest. On the authority of a distinguished firm of pharmacists, it is said that the practical difficulties of turning a laboratory experiment into a commercial process are so great, that for a long while to come we must continue to look to Nature for our supplies of the fragrant acid. Her Majesty's Ambassador at Berlin writes in the same strain, but with less confidence. The Director at the Manufactory of Chemical Products at Thann, in Alsace, assures her Majesty's representative, through the German Foreign Office, that "certain

difficulties have been encountered which must be overcome before there can be any question of the mercantile utilisation of the process in question." But he also states that the result of experiments "increases the prospect of ultimate success."

This is only another straw showing that the wind is blowing unfavourably to the simple pursuit of agriculture. The sails of farmers and fruit-growers, of gardeners and foresters, must be trimmed scientifically if the ship is to move. In earlier times "to plough and sow, to reap and mow" according to traditional methods was sufficient. In a good season the kindly earth yielded abundantly; in a bad season a hungry population gave a better price. But now, facilities of transport equalise, and therefore minimise, the varying results of the weather all over the globe. And still more, the rapid advance of scientific methods is dogging the heels of slow-moving Nature. Natural products of every kind are being produced more rapidly, more cheaply, and more certainly by scientific aid. "Protection," "Bimetallism," and a dozen other remedies may be beneficial or harmful; it is not within our scope to discuss them. But we lay down an inevitable and indisputable proposition. Unless the agriculturists of England and her colonies choose to train themselves in scientific methods and to banish for ever their easy opportunism, our agriculture will be ruined.

Science and Art.

If Science renders service to those that supply the practical needs of the world, no less can she aid the ministers of its more ethereal pleasures. The debt that the fine arts owe to Science was dwelt upon a few years ago by Dr. E. du Bois-Reymond, whose address, recently republished in the Smithsonian Report for 1891, we have just had the pleasure of re-reading in a copy of that Report sent to us. But it is only at the close of this interesting disquisition that the eminent physiologist asks the question, "What have sculpture and painting been able to do for science in return for its various services?" And then he will hardly stay for an answer. In fact, the difference between the artistic and scientific modes of approaching nature is so great, that we fear no very satisfactory answer can be given. Even in so obvious a sphere as the representation of natural objects, artists, working to the end of art, have, as a rule, not produced results sufficiently reliable to serve the turn of Science; while to refer under this head to the more or less accurate drawings intended to elucidate scientific writings would be a gratuitous insult to Art. Such notable exceptions as have existed only make more manifest the general prevalence of inaccuracy on the one hand and absence of artistic feeling on the other. If some return to nature has been shown by our English artists of this century, oddly enough largely in consequence of the teaching of that very Ruskin who deplores the pernicious influence of science, and calls forth the scorn of Professor Du Bois-Reymond, yet the present

tendency seems to be in quite an opposite direction. Some of our *fin-de-siècle* young men, influenced, it would seem, by Japanese impressionism and Japanese grotesqueness, seem to have forgotten the chief ideal of the Japanese artist, who, like Caleb Plummer, "tries to go as near to nature as he can for sixpence."

An interesting example of the assistance which the accurate yet delightful art of Japan may render to the scientific enquirer occurs in a paper by Dr. C. Schlumberger in the last number of the Memoirs of the Zoological Society of France (vol. vii., p. 63, and pl. ii.). The so-called dancing mice of Japan are a breed as to the origin of which there has been some little discussion. The peculiarity of these animals is that they twist rapidly round, apparently in pursuit of their own tails, as kittens or puppies are sometimes made to do with us. This dervish-like habit is supposed to be due to a crook in the brain, a mild insanity perhaps produced by some artificial selection or cross-breeding. Now, Dr. Schlumberger has come across a Japanese *netsuké*, the carved ornament of a tobacco-pouch, which represents a family of these dancing mice. The father and mother and eight little ones are reproduced with the utmost minuteness in characteristic attitudes. The chief interest of this work of art lies, however, in its colouring. The father and mother and four of the young ones have white coats spotted with black; two of the young are quite black and two quite white. The mother and the two white young have pink eyes, while those of all the others are black. Dr. Schlumberger therefore infers that the dancing mice of Japan are the product of artificial crossing between well-defined melanic and albino varieties.

In Old Japan.

A propos of Japan, Mr. F. A. Bather, whose recent report on Natural Science in that country will be remembered by our readers, favours us with the following note:—

"Mr. Kumagusu Minakata, whose knowledge of Oriental science has often been shown in his interesting letters to *Nature*, points out that, in the course of transcribing my notes, I have confused the date of Kaempfer's birth with that of his arrival at Nagasaki. He was born in 1651, and first visited Japan in 1690. Consequently the interval between Kaempfer and Siebold was not 175 years, as stated on p. 24, but 135. . . . Some observations condensed from Mr. Minakata's letters to me may serve to supplement and correct my hasty sketch. So far as the past is concerned, the title 'Natural Science in Japan' should be held to refer to European science, and not to the knowledge that the Japanese themselves had of their own lands and seas. Crude as it was, yet the Japanese, imbued with Confucian maxims, vivified by poetical learning, excited by magical formulæ and anxious for medical knowledge, had a Natural Science of their own, which, though borrowed from the Chinese, was by no means unimproved since its transportation. For instance, Li Shi-Chin's 'System of Materia Medica'—of which the Japanese naturalists were the commentators, as the mediæval scholars of Europe were of Aristotle and Pliny—placed such parasitic phænogams as *Gastrodia*

and *Orobanche* in the division 'Mountain herbs.' The Japanese scholars, though they might be childish admirers of the Chinese authorities, did not hesitate to remove these plants to another division, 'the Fungi.' Though this may seem laughable to modern botanists, still it implies the existence of the idea of parasitism or saprophytism, a conception which must have been based on long-continued observations; and it must be remembered that a European taxonomist so recent as Lindley esteemed physiological functions as of major importance in the arrangement of the vegetable kingdom. Thus the germ of science was not utterly wanting among the naturalists of old Japan. In the days of Kaempfer, it is true, the naturalist, properly so called, was very rare in Japan, the few that existed being rather Confucianist or medical scholars. But when Siebold arrived, the naturalist had already been developed by the efforts of many, among whom Ranzan Ono was conspicuous. This man, eulogised by Savatier as 'le Linné du Japon,' has left as his memorial the great 'Guide to the Study of Materia Medica,' which embodied his researches among Japanese and Chinese literature on botany, zoology, and mineralogy. Iwasaki Tsunemasa, mentioned on p. 25, was his pupil.

"To the causes that I mentioned as hindering the progress of science in Japan, Mr. Minakata adds the inattention of Europeans to what was and is doing in that country. This it was the aim of my paper to help to counteract."

THE HONGKONG PLAGUE.

WE cannot leave the Far East without referring to the plague now decimating the Chinese dwellers in our city of Victoria.

Among us modern sanitation seems to have stamped out some of the deadlier pestilences that once prevailed. In London and most of our large towns, typhus fever, at least in epidemic form, is almost a thing of the past, and it may be doubted whether the oriental plague could obtain much foothold in this country, even if conveyed hither. There is little reason to doubt that this epidemic is identical with the Black Death of the fourteenth century, and the Plague of the seventeenth. As in the case of typhus fever, the essential conditions for its propagation appear to be filth and overcrowding, and in these respects China is probably worse than was our own country in the Middle Ages. That the virus is a living organism few will doubt, and it must be an inspiriting thing to the mind of a bacteriologist to have the chance of studying a mediæval pestilence with appliances and methods of the most approved modern kinds. Professor Kitasato, of Tokio, is already on the spot, and is asserted to have discovered in the glandular swellings a bacillus characteristic of the disease. The details of his observations will be awaited with interest by pathologists, since few could have hoped that the Black Death would ever be traced to its ultimate cause in so modern an organism as a bacillus.

SEWER AIR.

TURNING to similar dangers nearer home, faulty drains and sewer air are among those regarded by the householder with especial

abhorrence. His surprise will probably be no greater than his satisfaction when he learns from the Reports on Sewer Air recently presented to the London County Council by Mr. J. Parry Laws, that this enemy is in many respects less black than has been painted. Mr. Laws' experiments only confirm the results previously obtained by others in this country and abroad, but they will appeal to the Londoner with particular force, as having been carried out in some of the oldest, and, in some instances, the worst of his own sewers. They deal mainly with the number and character of the bacteria of sewer air as compared with those of fresh air taken at the same time in the vicinity, under normal and abnormal conditions. Normal sewage contains enormous numbers of micro-organisms—from four to six millions or more to the cubic centimetre—and it might have been expected that air in prolonged contact with such decomposing and fermenting sewage would have become highly charged with bacteria. In most cases, however, Mr. Laws found, as others had found before him, that fewer micro-organisms were present in sewer air than in fresh air taken at the same time, and, which is more important, that those present were related to and apparently derived from the outside air, and not from the sewage. No effect on their number seemed to be produced by moderate splashing, or, in an experimental sewer, by a considerable increase in the velocity of the air current, or even by drawing air through a sewer which had been kept empty for twelve days.

A mere knowledge of the number of bacteria present is of less moment than an insight into their nature, and an attempt was made in every case to determine the species present. Not only were no pathogenic species found, if we except one or two feebly pathogenic *Staphylococci*, which would not harm us if they could, but the bulk of the species present were of a positively ornamental character, and we read with pleasure that our sewers are inhabited by *Sarcina aurantiaca*, *Bacillus aureus*, and *Micrococcus candicans*. If this be so, it must be admitted that, whatever the injurious effects of sewer air may be, we have no experimental evidence of the presence in it of specific germs derived from the sewage. And in this connection may be noted the well-known fact that sewer men are not more liable to zymotic disease than other members of the community. The baffled hygienist must hence take refuge in hypothetical poisons of a chemical nature if he wishes to explain the deleterious effects of sewer air. The evidence of such injurious action is so strong that it cannot be ignored, and it may well be that the effect is a secondary one, and produced by lowered powers of resistance to specific poisons derived from other sources.

Science and Literature.

Meanwhile we have strayed rather far from Professor Du Bois-Reymond and his views on art. Let us hark back for a moment, for

there were some closing thoughts that caught our attention. A strictly scientific paper need not, he says, be without its artistic aspect; it may "be made as finished a piece of writing as a work of fiction." The qualities demanded are not beauty of phrase and aptness of metaphor, but lucid language, logical arrangement, and a due subordination of the various observations to the conclusion of the whole. "To strive after such perfection will always repay the trouble to men of science; for it is the best means of testing whether a chain of reasoning is faultlessly complete." The popular exposition of science may however, retain the ornamental character of literary art to a greater extent. The literature of knowledge, as a brilliant literary critic of the *Athenæum* calls it, may for these purposes invoke the artifices of the literature of power. The modern rush after scientific and technical education has brought with it an aversion to the other aspects of culture; and the practical pedant of to-day scoffs at literary form as unnecessary, and at the graces of style as "high-falutin." Against this tendency we have always protested, and shall continue to do so. For to see the material world alone is to be blind to one-half of nature.

But if we would ally Science with Literature, let us at least ask that the friendship be mutual; for so then will be the benefits. "Everyone," writes Sir Mountstuart Grant-Duff, "must be conscious of the curious effort in much modern writing to supply the absence of fresh facts and ideas, by saying old things in a new and much more difficult way. For a moment the strange contortions of the writer attract our attention; but it is presently found that his performance is a mere acrobatic feat, proving nothing more than the presence of a certain cleverness. The mind of the reader is neither enriched nor soothed. There is but one remedy, and that is to greatly increase the number of facts with which literature deals." The new facts with which Literature is called upon to enrich her store are those of Nature and Natural Science. "Literature will, in her turn, repay with interest all she gains from a larger commerce with Nature."

Food for Babes.

The above remarks of Sir M. E. Grant-Duff are quoted from an introduction that he has written to "A Handbook to the Study of Natural History for the Use of Beginners," which has recently been edited by the Lady Isabel Margesson, and contributed to by many writers. With the aims of this book we have much sympathy. At the same time, we must confess that it is not a little difficult to discover what its exact aims may be. Taken as a whole, the book would appear to be designed as the Natural History Codex of the P.N.E.U., that admirable body to which we have occasionally alluded. But the contributors to it seem to have had very different ideas of the way in which they were to fulfil their task. Some have clearly written for the parents; others as clearly for the children.

The consequence is a book that we should rather hesitate to put in the hands of our own children. For instance, we find it written that adaptation, mimicry, and the like "afford us beautiful opportunities for teaching both spiritual and moral lessons, without objectionable preaching, which go home to the hearts of children in such a way that the lesson, once learned, is never forgotten. For we must remember that children easily get hard and callous when left to themselves," and so on. It would never do to let a child behind the scenes in this way. On the other hand, if the book is for adults—even for parents—it is hardly necessary to tell them how to degrade the study of sea-weeds by turning them into Christmas cards.

We regret that we cannot speak better of this book, for many of the articles are useful in their way, and are on the whole free from the inaccuracies that disfigure most popular works of the kind; for this, indeed, the names of the writers are in most cases sufficient guarantee. Still the undoubted ability of some of the writers renders their articles the more disappointing. That on Zoology, for example, consists of lists of books far too comprehensive, interlarded with this sort of thing,—"Gradually we must pass from considering the bird as an intact unity like one of ourselves, to see it as a marvellous living engine, with many parts or organs, as a great web of tissues, as a vast city of cells—competing and co-operating, and finally as an ensouled whirlpool of living matter, which, though ever changing as streams of matter and energy pass in and out, yet retains its integrity till death comes." We don't wish to use the word "highfalutin" so soon after uttering a *caveat* against it, but this really is a trifle "tall." Then again "The Study of Mosses" is doubtless most fascinating; but who would be attracted thereto by these extracts from the description of a Moss plant? "Its base is fixed in the soil by numerous fine filaments, ... some of these may be above ground and green—these are the *protonema*; others are brown, and ramify in the soil—these are the *rhizoids*. The fruit, to which the term *sporogonium* is applied, consists of a *capsule* containing *spores*, placed either at the end of a stalk, the *seta*, or else sessile among the leaves." The chapter on Fungi is nearly as dreadful.

We can only hope that the parents who are expected to read this book will be able to distil from it something that shall be more suitable than the above extracts as food for babes.

The Attack on the Pole.

The present year will be a marked one in the annals of Polar exploration. Never before have so many expeditions been in the field, and never before have the resources of modern science been so called upon to deserve, if not to command, success. The account of the preparations for the Wellman expedition, with its aluminium

boats and bridges, read like a chapter from Jules Verne, and exemplified the imagination of the American journalist. The news recently brought by the "Saide" concerning this expedition is a little contradictory; for while we are told that the "Ragnvald Jarl" was only six days on its passage from Tromsö to Danes Island, Spitzbergen, and that the sea to the north of that island was remarkably free from ice, on the other hand, we learn that, in the opinion of Captain Johannesen, of the "Smeerenburg," the "Ragnvald Jarl" must have been subsequently beset and crushed by heavy pack-ice. This does not augur well for the immediate prospects of the Jackson-Harmsworth expedition, which, in the opinion of Arctic experts, is in any case starting six weeks too late. Since, however, the party are prepared for an absence of two or three years, a slight delay at the outset will perhaps not affect the ultimate result of the expedition. It is, besides, far more the object of Messrs. Jackson and Harmsworth that worthy scientific results should be obtained than that the Pole itself should be seriously threatened. The expedition is very thoroughly fitted out with scientific instruments, and carries besides several experienced observers, although we understand that at the last moment some difficulties arose with regard to the latter.

Almost simultaneously with the starting of the "Windward," the Peary auxiliary Polar expedition left St. John's on board the steamer "Falcon" for Inglefield Gulf, Greenland, to bring home Lieutenant Peary's party. The vessel will call at Carey Island, where the unfortunate Swedish naturalists, Björling and Kallstenius, came to grief in their schooner the "Ripple" in 1892. Dr. Ohlin, a Swedish zoologist, accompanies the expedition chiefly for the purpose of searching for his two countrymen or their remains. The party on board the "Falcon" is also charged to explore Jones's Sound and to make a chart of the coast. After this it will call for Mr. Peary, and return to St. John's about the end of September.

Whatever else may happen, as all these expeditions intend to observe the aurora on one definite plan, the results to meteorology cannot fail to be of exceptional interest.

The Pollination of the Yucca.

In May, 1893 (Natural Science, ii., p. 321), we described in detail Trelease's studies on the pollination of the Yucca by the larvæ of the moth *Pronuba yuccasella*. In the Fifth Annual Report of the Missouri Botanical Garden (p. 137), Mr. J. C. Whitten has completed the details, previously incomplete, of the life-history of this interesting insect. He finds that the larvæ make their escape from the capsules, and enter the soil, during the rainy weather when the ground is softened and easy of penetration. He also makes the interesting notes that they do this during the daytime or at night, and not exclusively toward the end of the night, and that they drop quickly down to the ground by means of a silken thread.

I.

The Evolution of the Thames.

PROBABLY nothing has gained for geology a greater number of recruits than a love of the scenery and a desire to understand the origin of the physical features of the district in which one dwells. The arrangement of river systems has been an especially useful irritant; for no one provided with eyes and wits can travel about England without being puzzled by the singular anomalies in the courses of our rivers, which often appear to have been planned before the law that water should manage to find its own level had come into operation. The Exe, for example, rises on the north coast of Devonshire, almost within stone's throw of the Bristol Channel, from which it is separated only by the narrow "Hobby Drive"; nevertheless, it flows right across the county and enters the English Channel, instead of cutting its way to the shore near at hand. The fact that no one is allowed to go through the Hobby Drive without paying sixpence, seemed at first to supply a possible explanation, since as the river either would not or could not pay the toll, it had to go round another way. In other parts of the country the rivers do exactly the reverse, having cut their way through hills when an easy course lay open to them round the flanks. The desire to get at some explanation of these eccentricities which did not assign an element of "cussedness" to the rivers concerned, has given the writer, and no doubt many others, a first interest in geology.

The history of the Thames has been a comparatively simple one, and it has not had to record the sensational revolutions of such rivers as the Rhine or the Hudson, though great differences of opinion still exist as to its age and formation. During the past four years, several very important additions have been made to our knowledge of the subject; but before considering these, it may be advisable to refer to the main geological sequence in the district so as to get a time scale for comparison. The western part of the Thames Basin is formed of a series of clays and limestones belonging to the Oolitic series. These are succeeded to the east by newer beds forming the Cretaceous system, of which the two most important members in this connection are the Chalk and some beds of chert formed of sponge remains occurring in the Lower Greensand to the south of the Thames.

Above the Chalk is a series of sands, clays, and pebble-beds forming the Lower London Tertiaries and London Clay: the latter is covered in places by sands and pebble-beds forming the Bagshot series. After this there is a great blank in the Thames Basin: when the geological record again commences, it does so only with a series of gravels, clays, and brickearths, which rarely contain any fossils except some derived from older deposits. In consequence, their correlation and methods of formation present problems of unusual difficulty. The following table summarises these beds in descending order:—[1]

I.—PLEISTOCENE.

(*a*) *Post-Boulder Clay.*—Brickearths and gravels at low levels in valleys of existing rivers. Most of the molluscs and mammals of existing species.

High-level terrace gravels and brickearths with remains of mammoth, arctic plants, etc.

(*b*) *Boulder Clay.*—Chalky Boulder Clay with fragments of rocks and fossils from the north of England.

II.—PRE-BOULDER CLAY, OF UNCERTAIN AGE.

Newer Plateau Gravels: Gravels containing many northern fragments and quartzite boulders.

Older Plateau Gravels: Gravels with many small quartz pebbles. The "Westleton Shingle" of Professor Prestwich and the Pre-Glacial Pebble Gravels of the Geological Survey.

"*Southern Drift.*"—Partly synchronous with above.

III.—PLIOCENE.

Crags of Suffolk and Sand on Chalk at Lenham in Kent.

IV.—EOCENE.

Pebble Beds of Bagshot series.

The most important recent attempt to solve the problems of the origin of the Thames Valley is that of Professor Prestwich in a series of three papers entitled "On the Relation of the Westleton Beds . . . . and their Extension Inland," published in the *Quarterly Journal of the Geological Society* for 1890. The papers are the result of over 50 years of work, and describe some sections that were closed from examination before the younger generation of geologists saw the light. They are a summary of a life's work by the Nestor of British geologists, and from this, as well as from their intrinsic importance, demand respectful notice.

Professor Prestwich's theory of the sequence of events in the south-east of England is as follows. In Lower Pliocene times a sea spread

[1] In reference to the above table it should be noted that the only bed positively assigned to the Glacial is the Chalky Boulder Clay. The gravels that underlie this are no doubt closely connected with it, but were obviously deposited or re-arranged by water. To avoid ambiguity in the article the term "Glacial" is not used as a time name; and beds are simply referred to as being earlier or later than the Boulder Clay instead of as Pre- or Post-Glacial.

across from Belgium to Kent, and thence to Suffolk, where it continued till Upper Pliocene times. In Kent, this period of submergence was soon brought to an end, before the Crags of Suffolk had been deposited, by the elevation of the great arch that once covered the Weald. As the land there rose the summit was denuded away, and the material was washed down the northern slope as the "Southern Drift." This began in the Upper Pliocene, or at the time of the Norwich Crag, and continued into the Pleistocene. North of the Thames the Pleistocene series began with a marine shingle beach, which extended from Norfolk into Berkshire, and possibly further. The pebble beds in the valley of the Bure, which were included by Mr. H. B. Woodward in the Norwich Crag, and are generally known as the Bure Valley beds, are taken by Professor Prestwich as the type of this marine shingle. He has traced it from the Bure across Suffolk to Braintree, and regards as its southern extension the high level pre-Glacial gravels marked in red on the maps of the Geological Survey. These occur scattered over the higher hills of south-west Essex, Middlesex, Hertfordshire, Buckinghamshire, and Berkshire. The land then rose, and the erosion of the valleys of the Lea, Colne, etc., commenced; at the same time, the climate became more and more arctic, until it culminated in the "Glacial Period." The Chalky Boulder Clay was then deposited in the district by an ice-sheet from the north. Still later the Chalk escarpment was formed, and the Thame and the Isis flowed northward along the valley at its foot, and entered the Wash. After this the Chalk escarpment was breached at Goring, and the basins of the Thame and Isis were connected with that of the Kennet, and thus formed the Thames. So, according to Professor Prestwich, the formation of the Chalk and Oolite escarpments, and practically the whole of the physiography and river systems of the south-east of England, date from Glacial and Post-Glacial times.

The theory is a brilliant and fascinating one, as it gives a definite historical classification of the drifts, and a working hypothesis with which to determine the relations of a series of gravels otherwise in chaos. Nevertheless, there seems a general consensus of doubt as to the three main positions in Professor Prestwich's argument—viz., (1) The original continuity of the isolated patches of pebble gravel which he has grouped together as the Westleton Shingle; (2) the recent date of the Chalk escarpment and its breaching by the Thames; and (3) the submergence of the whole of the South-West of England below the sea in Pleistocene times. In regard to the first, there can be little doubt as to the former continuity of the Westleton beds from the Bure Valley into northern Essex as far south as Braintree. Similarly, the gravel patches in the London district which Professor Prestwich has referred to the Westleton Shingle all agree in being Pre-Glacial in their general composition, and were doubtless deposited by a common agency at approximately the same period. But the

gap between these and the nearest of the truly marine Westletons is a great one: the most north-easterly of the patches in the London area is at Highbeech, at the height of 370 feet; the nearest of the Suffolk and northern Essex series is 30 miles distant at Braintree, where the gravels are at the level of 240 feet. There seems, moreover, a considerable difference in the gravels of the two areas: those of the northern Essex and Suffolk group are probably truly marine, whereas, in the case of those of the London district, there seems no evidence to refer them to the same agency, except their gradual rise when followed to the south-west. The acceptance of the correlation and original continuity of the two sets of gravels in these two distant areas is essential to Professor Prestwich's hypothesis. It is almost impossible actually to disprove this, but the evidence in its favour is at present very limited.

The view that the whole of the South-East of England has been submerged below the sea in Pleistocene times is also one that has not found much favour. Messrs. Monckton and Herries, for example, certainly do not accept any submergence of the district in Essex around Brentwood and Warley since Eocene times (no. 3, pp. 22, 23; nos. 4 and 5). It may be objected that the patches of sand on the North Downs of Kent and Surrey, which are referred to the base of the Pliocene series, prove a subsidence at this period; but as far as I am aware, the only one of the patches certainly marine is that in the neighbourhood of Lenham, east of Maidstone. This may indicate merely a gulf running from the Pliocene sea up the valley of the Swale. There is no proof whatever for the Pliocene age or marine origin of the sand patches elsewhere on the North Downs, such as that at Coulsdon.

Nevertheless, in spite of the general hesitation expressed, it seems impossible to get any one piece of direct evidence that is final against Professor Prestwich's conclusions. His theory is founded mainly on a series of inferences rather than direct proof, and inferential evidence is all we have against it. Dr. Hicks's (6) demonstration that the Hendon Chalky Boulder Clay mantles the surface and slopes of Pre-Glacial hills and valleys, and Mr. T. V. Holmes's discovery of Boulder Clay at Upminster (8), showing that at least two-thirds of the Thames Valley had been eroded before the Chalky Boulder Clay was deposited, are neither of them final; for Professor Prestwich fully admits a considerable amount of erosion before the Chalky Boulder Clay, but later than his early Pleistocene Westleton Shingle. Both the cases described by Dr. Hicks and Mr. Holmes are to the south of the Westleton line, and far from the great Chiltern escarpment, on which the point in dispute must turn.

There are, however, two areas which give strong *à priori* evidence against the truth of Professor Prestwich's theory. These are the Chiltern Hills and the Downs of western Berkshire, the structure of which we will now proceed to consider.

The most instructive area in the Chilterns is that in the neighbourhood of Tring, Prince's Risboro', the Wycombes, and the valleys of the Missbourne and the Chess. It is all included in sheet no. 7 of the maps of the Geological Survey, of which the "Drift Edition" admirably shows the complex series of gravels in the district. These are also described in two of the Survey Memoirs (1). The map shows that the Chilterns consist of Chalk, covered by a varied series of drifts, intersected by a number of valleys which cut across the ridge from north-west to south-east. The most extensive of the drifts is a series of gravels and brickearths that are coloured pink on the map and marked as "Glacial." There is no Boulder Clay in this immediate district, but the gravels, though somewhat different in composition, appear to be continuous with those which pass under this deposit further to the east. They are, therefore, earlier than the Boulder Clay. Though marked as "Glacial," no one would be likely to maintain that they are Glacial in the same sense as is the Boulder Clay. They are certainly due to water action in some form, instead of to the direct agency of ice like a moraine. To avoid the ambiguity involved by the use of the term "Glacial," it may be advisable to call these the "Newer Plateau Gravels." They must be distinguished from another set, which may be called the "Older Plateau Gravels," which are the "Pre-Glacial Gravels" of the Geological Survey and the Westleton Shingle of Professor Prestwich. Examples of these occur in this area capping the outliers of Eocene beds at Penn and Lane End, near High Wycombe. The former existence of these two plateaux, each capped by its own set of gravels, was first demonstrated by Professor T. McKenny Hughes in 1869 (9).

A third set of gravels occurs on the floors of the set of valleys running through the Chalk ridge; as these occur right up to the watershed, it is clear that they must have been deposited at a time when the valleys continued further to the north.

The fact that the valleys of Bradenham, Hampden, and the Miss cut through the plateau and contain none of the "newer high level plateau gravels" shows that the valleys are later than the gravels. The valleys themselves, though now dry in their upper parts, breach the Chalk escarpment, and it is impossible to examine them without feeling that they are due to erosion by rivers that once rose some distance to the north. The breaches made by the heads of the valleys are most impressive; thus, that on the road from Wendover to Aylesbury crosses by a pass the summit of which is 503 feet high, while the ridge on either side rises to 790 and 800 feet. Similarly, the road from West Wycombe to Prince's Risboro' crosses through a breach at the height of 427 feet between points of the ridge that are 700 feet only three-quarters of a mile to north-east and south-west. The deposits on the floor of the valley are also suggestive and tell no less strongly in favour of the formation of the valleys of

former streams, for beds of gravel lie along them. Sections in the gravels on the floors of the dry Chalk valleys are scarce and unsatisfactory; there is a small one within the fork of a lane and the main road from Missenden to Prince's Risboro', just to the north-east of Hampden House. The hole shows a coarse gravel of unrolled flints with but little sand and without any oolitic material. Mr. Whitaker records a note by Mr. Jukes Browne upon the gravels at Saunderton in the next valley to the west, which show the same characters (1, p. 447.)

The nature of these gravels is important, as it is to their constitution that we have to trust to know what was the former course of the rivers that eroded the valleys in which they occur. There are two possible explanations: either these streams rose on the Oolites to the north, and are earlier than the Chalk escarpment, or they were confined to the Chalk (as the Chess still is), and existed when this formation extended much further to the north-west than at present. That this latter view is correct appears the more probable, as the gravels of these dry valleys are composed solely of material derived from the Chalk and the "pink" gravels which overlie it. If the head-waters of the rivers had been situated on Oolitic rocks, it is about certain that some fragments of these would occur in the gravels. It might be suggested that these have been eliminated owing to the distance of the valleys from the Oolites; but when we remember that fragments of these rocks occur in the Thames gravels on the south side of the Chalk, and much further from the Oolites than the Saunderton—West Wycombe valley, this explanation seems insufficient. That the valleys are later than the so-called "Glacial Gravels" or Newer Plateau Gravels, we have already seen; but the very irregular nature of the floor of the valley, from which all traces of any former channel have been destroyed, shows that it is of some considerable age. To learn whether the valleys were earlier or later than the Boulder Clay, we must turn to a member of the series which cuts this deposit. The Lea serves our purpose very well; in its upper course it presents identical features with those of the Miss and Chess, while we know that at Hertford the Lea Valley was eroded in the period that intervened between those of the Newer Plateau Gravels and the Boulder Clay, deposits of the latter of which occur within it. If therefore, as seems highly probable, all the north-west to south-east valleys through the Chilterns were formed simultaneously, then the Chalk escarpment was breached in the period between the formation of the Newer Plateau Gravels and the Boulder Clay.

The next point for consideration is as to the evidence to show the age of the escarpment itself. The distribution of the Boulder Clay gives us a good hint in this direction. This deposit was apparently formed by the southernmost portion of the East Anglian ice-sheet, which travelled from north-east to south-west: towards its end it appears to

have split into two tongues, one of which kept to the south of the Chalk ridge, as far as St. Albans, Hendon, and Finchley, while the other flowed along the foot of the Chalk escarpment past Hitchin and Dunstable, whence it trended off to the north-west towards Leckhampstead. The Chalk hills of this district thus rose above the level of the ice which lay upon their lower flanks.[2] This tongue of the glacier seems to have had but little erosive power and to have flowed only along lines of low land previously in existence. Thus in the valleys of the Brent and the Lea it simply occupied pre-existing valleys without eroding fresh ones. As, moreover, it was apparently unable to surmount the Chalk ridge, it does not appear likely that this thin lobe of ice eroded the valley it occupied on the north side of the escarpment. It is much more probable that it there extended so far south owing to the former occurrence of a valley along which it flowed. That is to say that the valley at the foot of the Chalk escarpment, and the escarpment itself, are both older than the Boulder Clay.

We have next to consider the question whether the Thames ever flowed up the valley of the Thame, across the watershed, and down the Ouse into the Wash. Conclusive evidence against this can only be obtained by a detailed study of the distribution of the gravels upon the watershed between the Thame and the Ouse. Until such evidence is forthcoming we are thrown back on two general considerations: in the first place, the great irregularity and sinuous course of this watershed renders it very improbable that it could have been formed by a line of elevation which broke across the former course of a river. Further, the only way in which the Thame could have surmounted the barrier would have been by its waters having stood at a higher level; there is no evidence of any great lake which discharged to the north or of movements of the country which have since lowered the valley of the Thame. Apparently Professor Prestwich's only reason for suggesting this connection between the Thames and the Wash was the difficulty of explaining the formation of the gorge through the escarpment between Moulsford and Pangbourne. But this gorge is simply the southernmost of the series of parallel valleys through the Chilterns. The level of the floor of the gorge is lower than that of the passes through the escarpment further to the north-east, because the height both of the ridge and the passes descend as the former is followed to the south-west. The Thames gorge is so closely analogous to the series of valleys parallel to it, that it appears most probable that it was formed at the same time—viz., before the period of the Boulder Clay. We may summarise, then, the sequence in the Chilterns as follows:—

[2] Mr. Worthington Smith has recently described some drift deposits on the higher parts of the Downs near Dunstable as a "Boulder Clay." This has not been generally accepted as such; and even if it be glacial in origin, as it contains no northern boulders, and is exclusively composed of local materials, it does not prove that the northern ice-sheet ever occurred on the higher parts of the Downs.

(1.) Existence of a high plateau; formation upon this of the "Older Plateau Gravels" of Lane End, Penn, &c. (Coloured red on the Geol. Surv. Maps.)
(2.) Prolonged period of denudation, resulting in the formation of a second plateau, lower than the last, and interrupted at places by hills left undenuded. Deposition on this plateau of the "Newer Plateau Gravels." (Coloured pink on the Geol. Surv. Maps.) The Chalk escarpment was then in existence, but it was much higher and further to the north.
(3.) Erosion of the series of north-west to south-east valleys, including those of the Thames at Goring, and of the Miss, Chess and Loudwater.
(4.) Cutting back of the Chalk escarpment to approximately its present position by the erosion of the Thame and Ouse. This cut off the head waters of the Miss, Loudwater, etc.
(5.) Advance of the ice-sheet which deposited the Boulder Clay.

The only remaining question is whether it be possible to fix any definite date for this series of events. Everyone will admit that no. 5 was early Pleistocene, as it is generally considered that the arctic facies of the fauna in the Norwich Crag was due to the refrigeration of the climate, which culminated in the formation of our English glaciers. This gives us the latest date for the last member of the series. Professor Prestwich includes no. 1 also in the Pleistocene; but if we do not accept his correlation of these drifts with the Westleton beds, then these gravels may be of any age between the Eocene and the early Pleistocene. When we remember how little the structure of the country appears to have altered since the time of the Boulder Clay, it is a rather heavy order to crowd such a long series of events into the short time between the end of the Pliocene and the beginning of the deposition of the Boulder Clay. This affords further reasons against the acceptance of the Westleton age of the "Older Plateau Gravels," which may be allowed an extension backward into the Upper Tertiary, the exact amount of which cannot now be determined.

Let us now turn to the western end of the Berkshire—Chiltern escarpment, where valuable evidence of the Pre-Pleistocene age of the Chalk escarpment is afforded by the distribution of the Sarsen stones. These are huge masses of siliceous rock, now lying scattered over the Downs. Whence they came is not quite decided: they unquestionably represent some former bed of sand, which has now been denuded away and these boulders only left. They vary greatly in composition. They are believed to have been derived either from the Woolwich and Reading or the Bagshot beds, but probably came from more than one horizon. Some of them have been found *in situ* in the former, and thus unquestionably belong to them, while others occur on the London Clay and on Bagshot beds, and thus with equal certainty are far more recent than the Woolwich and Reading beds.

Some, moreover, occur in districts where it is probable that this formation never extended.

The distribution of the Sarsens enables us to approximately determine the original extension of the Tertiary Sands that yielded them. The Sarsens are abundant on both sides of the Kennet valley, on the Marlborough Downs, and thence along a line past Avebury north to Swindon. The blocks here are small, as they are also at Highworth and Hannington. They occur on the dip slope of the Chalk of the Berkshire Downs as far as Aldbourne and Lambourne, but they appear to be entirely absent from the higher part of the Downs. In a walk from Ashbury to Aldworth, along the edge of the Chalk escarpment past Uffington Castle and White Horse Hill, not a single Sarsen stone was seen, except those forming the cromlech and circle of Wayland Smith's cave and the famous "Blowing Stone." These are certainly not *in situ*: the Blowing Stone has been moved to its present position. Nor can I find any record of Sarsens on the escarpment. The highest point at which they occur is one mile from the edge of the Chalk escarpment. Mr. W. Cunnington, who knows the Wiltshire and Berkshire Downs better than anyone, says that their absence from the ridge is certainly not due to their having been removed by man; and my colleague, Mr. Andrews, has recently searched the downs around Aldbourne and can find none nearer the escarpment than on Kingstone Down. Their absence from the escarpment seems to show that the Sarsen-yielding horizon thinned off against the higher part of the Chalk, so that this apparently rose above the level of the water in which these sands were accumulating. The estuary probably extended up the Kennet Valley and then over the lower slopes of the Chalk Downs to Abury, and north of this till it reached the Jurassic plain about Swindon. A further continuation may be indicated by some Sarsens which occur in the valley that runs from Swindon to Hannington and near Highworth. But in any case the Sarsens in the Swindon district are sufficient to prove that at least a part of the Berkshire Chalk escarpment was in existence in the time of the later set of Sarsen-yielding sands, *i.e.*, probably Upper Bagshot.

That the Berkshire escarpment took up its present position earlier than that of the Chilterns is shown very clearly by a comparison of their structure. (Figs. 1 and 2.) Thus, while the Chilterns is jagged and irregular and breached by numerous former stream courses, the Berkshire escarpment is regular and unbroken. The whole drainage system of the Chilterns has been altered since the date when the escarpment was formed, while in the Berkshire Downs the drainage is entirely resultant on the present slope of the country; thus the drainage of the latter is of the type known as subsequent, that of the former as antecedent.

There is a third line of argument which there is only space here briefly to indicate, viz., that the Thames and its tributaries

are essential members of the river systems of England to the south and east of the Humber and the Severn, and that the main outlines of this system were determined long before Pleistocene times. If we examine a map of this portion of England (Fig. 3), we see that all the rivers rise in a central area included within a radius of about thirty miles from Rugby. Hence rivers flow north-east into the Wash and the Humber, south-west into the Severn, and south-east into the Thames. If we examine a sketch-map showing these rivers, two facts are clearly brought out: first, that the drainage lines are dependent on the ridges of the Oolites; and, secondly, that they appear to be independent of the arrangement of the highlands of the chalk in the Chilterns. Thus, while the Thames and the valleys of the Loudwater, Miss, Chess, and Colne run right across the Chalk at right angles to the ridge, and in a south-easterly direction, the rivers of the Oolitic country, on the other hand, flow in the main to

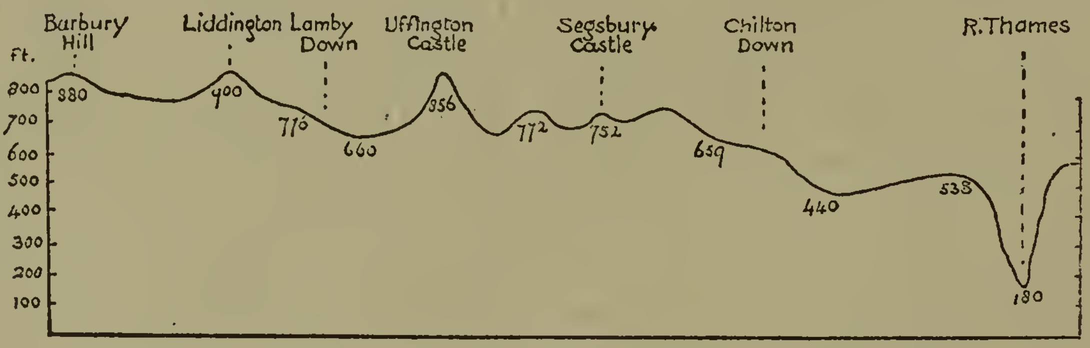

FIG. 1.—Outline of the Berkshire Chalk Escarpment.

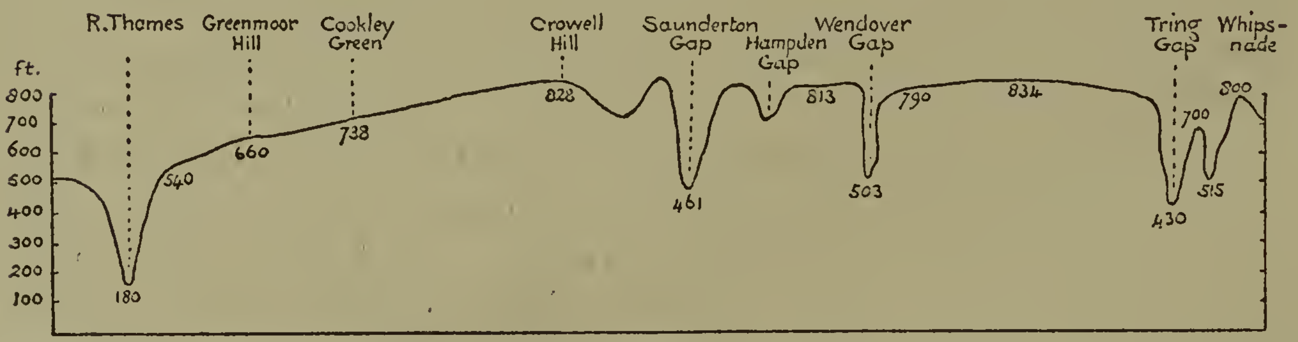

FIG 2.—Outline of the Chiltern Escarpment, showing the valleys of the Thames, Loudwater, Miss, and Colne.

north-east or south-west, parallel to the ridges. That is to say, the river systems of the Chalk country were antecedent to the hills, while in the Oolitic country they were subsequent or simultaneous. This admits of the simple and probable explanation that the rivers rose on a central plateau and radiated thence in every direction, flowing along the lines of least resistance. Those rivers whose course was parallel to the strike of the beds cut valleys through the soft clays, and left the harder limestones standing out as ridges between them. Those which drained the south-eastern slope had to cut their way at right angles to the strike, and thus formed deep narrow valleys through the Chalk ridge. At first these were probably all good sized streams, but the Thame and the Ouse which flowed in the valley between the Chalk and the Oolites cut the escarpment further and further to the south, and thus drained off the headwaters of the Buckinghamshire tributaries to the Thames.

The dates of these various episodes cannot be fixed very precisely. There are two outside limits: the valleys are earlier than the Boulder Clay. They are later than the Newer Plateau Gravels, but it is not possible to determine the exact age of these, for if we reject Professor Prestwich's hypothesis of their correlation with the Westleton beds, then they may extend back indefinitely into the Tertiary. The nearest approach to a determination of a maximum age of the great

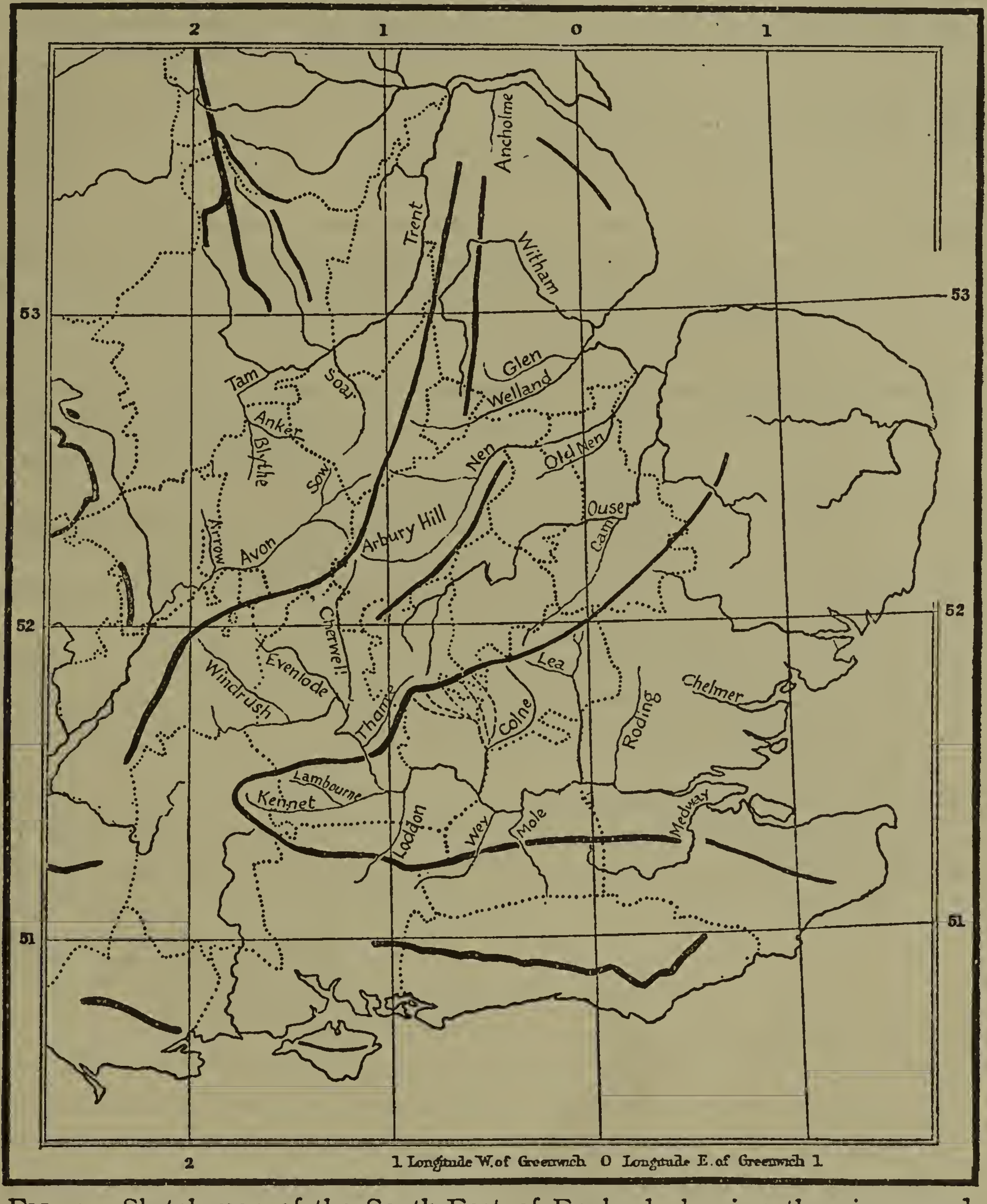

FIG. 3.—Sketch-map of the South-East of England, showing the rivers and principal escarpments (black lines); the dotted rivers are now dry.

central plateau which fixed the direction of the rivers of the south-eastern quarter of England is that it is Post-Eocene; how much later, one cannot be sure. The evidence quoted from the Berkshir area shows that the erosion of the main valley between the Chalk and the Oolites had commenced before the period of the later set of Sarsen stones, which are probably of Upper Bagshot age. So that one can only conclude that the main outlines of the river systems in

question were laid down in the long recordless period which followed the close of the Eocene, when England for the last time "arose from out the azure main." As no deposits of any value as a time scale have been left in this district for this period, it is unfortunately only too possible that we may never be able to arrive at a more exact determination of the date of the Thames and its northern tributaries.

PRINCIPAL REFERENCES.

1. **W. Whitaker.**—"The Geology of London and of part of the Thames Valley." *Mem. Geol. Surv.* 2 vols. 1889. (*See especially* pp. 183–194, 281–294, 299–303, 447–449, 496-499.)

   W. Whitaker.—"The Geology of Parts of Middlesex, Hertfordshire, Buckinghamshire, Berkshire, and Surrey." *Mem. Geol. Surv.*, explan. sheet 7, 1864.

2. **J. Prestwich.**—"On the Relation of the Westleton Beds or Pebbly Sands of Suffolk to those of Norfolk and on their extension Inland; with some observations on the period of the final elevation and denudation of the Weald and of the Thames Valley, etc." *Quart. Journ. Geol. Soc.*, vol. xlvi., 1890, pp. 84–181, pls. vii., viii.

3. **H. W. Monckton and R. S. Herries.**—"On some Bagshot Pebble-Beds and Pebble Gravel." *Proc. Geol. Assoc.*, vol. xi., 1889, pp. 13–23.

4. ————————————————"On some Hill Gravels north of the Thames." *Ibid.*, vol. xii., 1891, pp. 108–114.

5. ———————————————— **and H. B. Woodward.**—"Excursion to Brentwood." *Ibid.*, vol. xii., 1891, pp. lxii.–lxvi.

6. **H. Hicks.**—"On some recently exposed Sections in the Glacial Deposits at Hendon." *Quart. Journ. Geol. Soc.*, vol. xlvii., 1891, pp. 575–584.

7. ————"On the discovery of Mammoth and other remains in Endsleigh Street and on sections exposed in Endsleigh Gardens, Gordon Street, Gordon Square, and Tavistock Square, London." *Ibid.*, vol. xlviii., 1892 pp. 453–468.

8. **T. V. Holmes.**—"The new railway from Grays Thurrock to Romford: Sections between Upminster and Romford." *Ibid.*, vol. xlviii., 1892, pp. 365–372.

9. **T. McK. Hughes.**—"On the Two Plains of Hertfordshire and their Gravels." *Quart. Journ. Geol. Soc.*, vol. xxiv., 1868, pp. 283–287.

J. Walter Gregory.

II.

Some Account of the Gall-Making Insects of Australia.

THE very interesting paper on the formation of vegetal galls and their inhabitants in the November number of NATURAL SCIENCE has induced me to offer some notes of my observations on the galls produced in this country, and the conclusions I have arrived at as to their origin after close study extending over some years.

The researches of many of our entomologists have shown that the insect fauna of most countries is regulated by, and adapts itself to, its surroundings in a most remarkable manner. Though, in so large a mass of land as Australia, we have a great range of climate and a diversified vegetation, the bulk of this great island is covered with stunted, scrubby forest trees, broken here and there along the coastal mountains and alluvial flats by thick forest lands, and in the north-eastern parts by dense jungles and tropical bushes.

One can safely say that the trees prevailing over nine-tenths of Australia are "gum trees," Myrtaceous trees chiefly belonging to the genus *Eucalyptus*, and smaller shrubby trees belonging to the order Leguminosæ, genus *Acacia*, commonly known under the names of wattles, myal, mulga, etc.

In the genus *Eucalyptus* some 140 species are described, of which the extreme forms are the small, bamboo-like, slender-stemmed mallee-scrub gums, growing up in clumps from a common base, with thin, long, slender roots just below the surface of the dry, sandy country they cover in many of the inland districts, and the giant gums of Tasmania and South Gippsland, whose roots strike deep down into rich alluvial soil, and whose great rounded trunks shoot up 400 feet in height.

The Australian gum trees may be considered as taking the position in our forests that the oak trees occupy in those of England and Europe. As, therefore, on the oaks of the Old World the larger proportion of the most interesting galls are found, so in this country the bulk of ours are produced upon *Eucalypti*. On the western plains and uplands, but also in a less degree in the denser scrubs clothing the coastal ranges, bush fires that devastate miles and miles

of country are of annual occurrence. These, for the most part, do not destroy, but simply char or scorch the foliage of the larger trees, promoting a vigorous growth of young suckers shooting up from the base of the trunks or roots, the tender foliage of which forms a dainty lodgment for hundreds of insects, and is especially suitable for gall producers.

Before proceeding with a brief account of some of our most characteristic galls, we will take a glance at their food-plants, as certain groups seem to have a decided preference for allied species. The Cynipidæ are found upon Acacias and Eucalypts; the galls of the Psyllidæ upon the Eucalypts; the Cecidomyia (gall gnats) attack *Eucalyptus*, *Melaleuca*, *Leptospermum*, *Omalanthus*, *Acacia*, and *Frenela*; while though the greater number of our gall-making coccids confine themselves to *Eucalyptus*, some of the smaller groups also attack *Casuarina*, *Leptospermum*, and *Melaleuca*.

Both the form and the structure of the dipterous galls, all of which belong to the family Cecidomyia, are very variable. One of the commonest, *Cecidomyia Frauenfeldi*, Schurer, about the size of a small filbert, is composed of a number of papery scales overlapping each other like the petals of a rosebud, while the larva lies curled up at the bottom close to the point of attachment to the twig. Very different are the round, hard, shot-like galls of *Horotomyia Omalanthi*, Skuse, often covering the leaves with their rounded reddish warts. The structures of *Diplosis frenelæ*, Skuse, are very remarkable, being subglobular, fruit-like galls, formed of three thin valves growing upon the slender branchlets of the desert pine (*Frenela Endlicheri*).

Those that infest Acacias produce curious tubular galls. *Cecidomyia acaciæ-longifolia*, Skuse, aborts the young seed pods, altering them into irregular masses of tubular cells, each containing a larva, and occurring in such numbers that every seed pod on one of these trees is thus transformed.

Hardly anything is known about the Cynipidæ belonging to this country, but I have described three species (placed provisionally in the genus *Cynips*) all of which are found upon the Acacias, though I know several *Cynips* galls upon the Eucalypts.

Cynips acaciæ-longifolia attacks the flower-buds, aborting them into large rounded soft masses which, when mature, are handsomely tinted with red and yellow, from which they obtain the not inappropriate name of "Acacia apples"; *C. Maideni* produces galls upon the branchlets and twigs of the same Acacia, often altering them into swollen gouty excrescences six or seven times their natural size; *C. acaciæ-discoloris* deposits its eggs in the leaf-buds, which change into oval hollow galls ornamented with three short prongs or horns at the apex.

In the Homoptera, the family Psyllidæ contribute a number of rounded woody galls upon the leaves of the Eucalypts. In one species the larvæ form soft fleshy galls of a brilliant red tint, so

plentiful, sometimes, that the trees appear at a distance to be covered with crimson berries; another simply punctures the leaf and lives in the depression formed round it. This often swells out into an elongate, rounded, bubble-like gall still growing with the leaf after the Psylla has emerged.

I now come to the most remarkable group of our gall insects, namely, the gall-making coccids, belonging to the sub-family Brachyscelinæ. In the typical genus *Brachyscelis*, both the male and female coccids produce some of the most singular and fantastic growths ever found upon a tree. The members of this genus, if not all the Brachyscelids, confine their attentions to the Eucalypts. The male galls are generally small tubular growths from one to three lines in length, very slender, with a dilated rim or bell mouth at the apex; in some species they are very rare or unknown, while in others they cover the leaves in thousands. In *Brachyscelis munita*, Schrader, the male galls grow together, forming a curling, twisted mass of slender truncated tubes without an enlarged apex. I have received specimens of these from country correspondents under the name of "vegetable coral."

In *B. pharatrata*, Schrader, and in several allied species, the male galls are produced from the side of the fully-developed female gall, forming a flattened mass of coalescent tubes enveloped in a smooth fleshy covering, often of a brilliant red tint, ten or twenty times the bulk of the female gall from which it springs.

The larvæ are pale, yellow shield-shaped little creatures, with short stout antennæ surmounted by long bristles of irregular length; the segments are distinct and the outer margin of the whole insect is fringed with short feathery cilia, truncated at the apex, while there are two long hairs or filaments at the anal tip.

The male coccid is a delicate, pale yellow or bright pink, two-winged little insect, with long plumose antennæ and slender hirsute legs. The abdomen is very long and slender, and wonderfully adapted for reaching down to and impregnating the virgin females imprisoned in their woody coverings.

The female galls are produced upon the twigs or branches (exceptionally upon the leaves, as in *B. pharatrata*); they are round, conical, cylindrical, sessile, or stalked, some quite flat on the sides, others angular with apical extremities, produced into long straight or curved horns often several inches in length. *B. duplex*, Schrader, is four-sided, three inches long, an inch in diameter, with two sides prolonged into leaf-like horns from six to nine inches in length, gradually tapering to a point; the apical orifice in this gall is key-hole shaped, and lies between the bases of the horns. *B. munita*, Schrader, is smaller and much more variable; the apical orifice is a minute circular hole with a long, slender, straight or curled horn shooting out from each of its four angles. *B. ovicola*, Schrader, is always a symmetrical oval over an inch in length, while *B. pomaformis*, mihi, known in Queensland as

the "bloodwood apple," has a height of two inches, and a diameter of three.

There is a very curious double growth in *B. pileata* Schrader, and *B. variabilis*, mihi, unknown in any other species we are acquainted with. In the former, a long fleshy spike shoots out, under which the female gall is formed. As it increases in size the spike cracks round at the base, turning into an inverted cup-like covering protecting the young gall; this becomes brown and dry, falling off as the gall beneath reaches maturity. In *B. variabilis* the gall, which is very globose and woody, has a hollow dome-shaped false cell growing around and above the true solid gall. In the latter there is a very small round apical orifice, while in the dome above there is a ragged irregular aperture. Both these species appear to present a case where the outer bark of the stem throws off another gall on its own account.

The female coccids are top-shaped, round at the head, and sharply attenuated towards the anal tip or "tail"; they are yellow or semi-transparent "grubs" with no visible mouth or eyes when mature; short, rudimentary, three-jointed antennæ are situated on a small knob (evidently the head segment) between the fore legs; the middle and hind legs are slightly larger, and all terminate in a simple curved claw; the abdominal segments are distinct, covered lightly with fine hairs, and on the dorsal side carrying irregular rows of short sharp spines (which form very good guides for determining specific differences). From the tip of the anal segment stand out the anal appendages, consisting of two black horny processes lying close together, but tapering and generally cleft at the tip.

The mature coccid is simply a bag of semi-transparent jelly, which changes into a thin transparent fluid, in which the white opaque eggs can be seen, through the skin, floating about; they are extruded in long strings, each egg sac bursting a few seconds after its emergence, when the new-born larva, after a few kicks and struggles, shakes itself clear and crawls down under its mother, remaining inside the gall and often filling the cavity between the wall of the gall and her. After some days they crawl through the apical orifice and escape on to the tree.

If all the thousands of larvæ that escaped from a single gall were able to make good their footing upon the tree, that would succumb speedily to their united attacks; but besides the numbers that never succeed in producing a gall, many that do are destroyed at a very early stage by the attacks of hymenopterous and other parasitic insects.

There are two other genera belonging to this group, *Ascelis* and *Opisthoscelis*. In the former the adult female has no signs of legs or head, but is simply a mass of yellow jelly armed with a remarkable three-fingered anal appendage, which plugs up the apical opening in the flat blister-like or globular gall. The males of *Ascelis* do not produce galls, but remain in the parent gall until full grown.

In *Opisthoscelis* the adult female has the segmental divisions well defined, and though both the fore and middle legs are wanting there is a well-developed hind pair, in which the tarsal claw is produced into a long thread-like joint sometimes much longer than the whole coccid.

Another remarkable genus, *Cylindrococcus*, forms scaly fruit-like galls (which are frequently mistaken for seeds or cones) upon the "she-oaks" (*Casuarina*), while the members of the genera *Sphærococcus* and *Frenchia* form several curious galls very distinct from each other. It is doubtful if the last three genera can be included in the sub-family Brachyscelinæ, but they are mentioned here on account of the gall-making habits of most of their species.

From their minute size it is difficult to observe the larvæ of many of these coccids when upon the plants, but taking one as a type, *Ascelis præmollis*, Schrader, a species rather common about Sydney, found upon the "bloodwood" (*Eucalyptus corymbosa*), we may illustrate how the larvæ of Brachyscelinæ attack the foliage. Upon leaving the mother gall they scatter all over the leaves, but only those fortunate enough to come across the younger growth towards the tips of the branches gain a foothold. They attach themselves to the epidermis, simply sinking into the leaf, the larvæ disappearing in a very short time, leaving the apical orifice, like a pin prick, and a minute blister as the only indications of their presence.

In the case of a parent insect (as in the Cynipidæ) forming an incision in a growing woody tissue, it is quite possible that she might inject some acid or irritant secretion that would have a good deal to do with the ultimate form of the gall; but where the larva itself directly attacks the plant it is quite evident that there can be no irritant matter of any consequence introduced with it, and the final shape of the woody excrescence must be altogether influenced by the mode of feeding or working of the insignificant-looking little coccid.

It is, therefore, very remarkable that a number of white grub-like coccids, with aborted appendages, differing so little from each other in specific characters, should each have the power of forming a distinct and well-defined form of gall, which never varies in size or shape unless attacked by either outward or interior inquilines. The question then arises, What first causes a gall to be oval in one species, and in another species to put forth branching horns?

Their growth is very rapid, and the form of the gall is determined and apparent at a very early stage of formation on the tree. It is quite evident, in the genus *Brachyscelis*, at any rate, that the after-shape of the gall must be anticipated, because the inner skin, or wall of the gall, in contact with the female coccid, is smooth, hard, and sapless some time before its full development. Where the larva is a free-moving creature, it can no doubt materially direct the accumulation of vegetable matter either at the base, sides, or apex of the excrescence by feeding upon or neglecting one portion more than other, and

through an inherited instinct, each insect of a species forms a gall the exact counterpart of the other. This would be marvellous enough, but where the enclosed insect can apparently have no connection with her cell for a considerable portion of the latter part of her existence, it is even more astonishing, and the fact that the form of the galls is influenced at an early stage is borne out by my observations that many attacked by coleopterous and hymenopterous inquilines,[1] which have destroyed the coccid, still grow and increase in the uniform typical shape distinctive of its species.

The excessive flow of sap caused by the intrusion of the larva into the tissue of the leaf or twig would naturally cause an after-growth where the sap-circulation was altered, and naturally would form an excrescence of aborted, diverted, woody fibre, but this cannot account for a mass of galls with long curled horns or other decided characteristics being evolved from such a simple cause. The final shape of the gall must be directed by this tiny organism in some wonderful manner as yet unknown to us.

REFERENCES.

[The full descriptions and figures of the insects and galls referred to by Mr. Froggatt in this article will be found in the following paper by him.—ED.]

Notes on the family Brachyscelidæ, with some account of their parasites, and descriptions of new species. Part i. *Proc. Linn. Soc. N.S.W.*, ser. 2, vol. vii., pp. 353–372, pls. vi., vii., 1893.

Part ii. *Ibidem*, vol. viii., pp. 209–214, pl. viii., 1894.

Part iii. *Ibidem*, vol. viii., pp. 335–348, pls. xvi., xvii., 1894.

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[1 Inquiline: an animal that lives in an abode properly belonging to another, either at the expense of the latter, as in the present case, or merely as a co-tenant.—ED.]

III.

Books of Reference in the Natural Sciences.

OF all the services that can be rendered to the systematic naturalist, the compilation of books of reference is the most useful and perhaps the most thankless. The general value of these books as time-savers is so great, that it is wise in a journal that endeavours to assist scientific as well as general readers to devote a few pages to a list of those works in the various departments of Natural Science which are found by experience to be of service.

To anyone studying a particular group of animals or plants, the loss of time, to say nothing of temper, in hunting up references and getting special information is often considerable, and it must be a satisfaction to know that there is in existence a book in which we can find the greater part of the information for which we are seeking, simply by turning over a few of its pages. It may also, occasionally, raise in the searcher feelings akin to gratitude. Such books should be perfect to be of service; a wrong quotation shakes one's faith in the compiler, and if such often occur, the worker is apt to put aside the book with a sigh. Despite the difficulty of attaining this perfection, there are instances of accurate reference books—*e.g.*, Bronn's "Nomenclator Palæontologicus"—laboriously passed for press, and compared item by item with the original sources, after the manuscript is put into type; for this is the only way to approach exactness. In other cases, the compiler has been known not even to have seen a proof of his work, which has been allowed to go forth into the world with all its errors uncorrected. There can be little use for such books, though they do occasionally put one upon the track of something previously overlooked.

In the following list, no distinction has been made between good and bad books of reference; they have been classified under subjects as far as possible, and notes have been added whenever it has been considered necessary. The list, though obviously incomplete, has been compiled with the help of those most qualified to speak from experience and is offered to those who, working mainly in one field, occasionally wish to stray into another.

GENERAL LITERATURE OF NATURAL SCIENCES.

BRITISH MUSEUM.—Catalogue of Printed Books. This is now nearly complete for the following letters: A—P, U—Z. It is still going through the press, and will be completed in about ten years.

BRITISH MUSEUM.—Academies. An excerpt from *the* Catalogue, and containing all the publications of scientific societies entered under the name of town where published, contained in the British Museum up to 1885.

BRITISH MUSEUM.—Periodical Publications. An excerpt from *the* Catalogue, and containing all the serial publications, other than those of scientific societies, contained in the British Museum up to 1885.

ROYAL SOCIETY.—Catalogue of Scientific Papers. Up to 1883. This contains references to papers which appeared in scientific serials.

ENGLISH CATALOGUE OF BOOKS.—Yearly. Contains titles of all books published in Great Britain and the United States since 1835.

KAYSER.—Index Completissimus Librarum. 1834–1891. A list of all books published in Germany between those years.

LORENZ.—Catalogue général de la Librairie Français, 1840–1890. A list of all books published in France between those years.

BRUNET, J. C.—Manuel du Libraire. 8 vols. Paris, 1860–80. General.

TIELE, P. A.—Nederlandsche Bibliographie van Land- en Volkenkunde. Amsterdam, 1884.

ANDERSON, J. P.—The Book of British Topography. 1881.

LIDEN, J. H.—Catalogus Disputationum in Academiis et Gymnasiis Scandinaviæ et Finlandiæ. 1778–9. Continued by G. Marklin under similar title up to 1819. Upsala, 1820. [Most valuable for the older Scandinavian dissertations.]

DRYANDER, J.—Catalogus Bibliothecæ Historico-Naturalis Josephi Banks [Banks had a remarkable collection of early books on Natural History: this collection is in the British Museum (Bloomsbury), and is now incorporated in the general Catalogue.]

BÖHMER, G. R.—Bibliotheca Scriptorum Historiæ Naturalis. 1785. Valuable for early publications.

REUSS, J. D.—Repertorium commentationum a societalibus litterariis. Gottingæ, 1801. Gives papers on zoology, botany, and mineralogy published by academies, arranged under subjects.

NATURÆ NOVITATES (Friedländer & Sohn, Berlin).—This is a booksellers' fortnightly list, giving the titles of all publications on Natural History and the exact sciences as they appear.

HUTH & KLITTKE.—Societatum Litteræ. Yearly since 1887. List of current literature on General Science. Frankfurt a. O.

VALLÉE, L.—Bibliographie des Bibliographies. Paris, 1883.

LIST OF BIBLIOGRAPHICAL WORKS IN THE READING-ROOM OF THE BRITISH MUSEUM. British Museum, 1889.

SCUDDER, S. H.—Catalogue of Scientific Serials. 1885. Includes all publications of academies and societies.

BOLTON, H. C.—Catalogue of Scientific and Technical Periodicals, 1665–1882. 1885.

POOLE, W. F.—An Index to Periodical Literature, 1882; and supplementary volumes. All papers in magazines and similar periodical publications.

[HETHERINGTON, E.].—Index to the Periodical Literature of the World. Yearly since 1890. ("Review of Reviews" Office). Incomplete, but growing.

YEAR BOOK OF SCIENTIFIC SOCIETIES.—Yearly (Griffin & Co., London). Contains complete list of all British Societies with their officers, and details of publications issued during the year.

LEFEVRE-PONTALIS.—Bibliographie des Sociétés Savantes de la France. 1887. Published by the French Government.

MÜLLER, J.—Die wissenschaftlichen Vereine und Gesellschaften Deutschlands. 1883–1887. Comprehensive, but imperfect.

CATALOGUES of the libraries of the following institutions:—Royal, Linnean, Geological, Zoological, Museum of Practical Geology, Science and Art Department, South Kensington Museum, Royal Institution, Patent Office, Radcliffe, York Gate, and all such Catalogues, British and Foreign.

GENERAL ZOOLOGY.

THE ZOOLOGICAL RECORD.—Yearly since 1864. In the volume for 1893 will be

found a list of zoological serials with a note of the libraries where they can be seen.

Zoologischer Anzeiger.—A fortnightly serial containing original matter, but giving a list of new papers and books on zoological subjects as published. An index published in 1887, for the ten years then concluded, gives practically the whole literature of zoology during that time in one volume.

Anatomischer Anzeiger.—Similar to above, but dealing more especially with vertebrate physiology and anatomy.

Zoologischer Jahresbericht.—Yearly since 1879. Similar to the "Zoological Record," but deals with Morphology rather than Systematic Zoology.

Zoologisches Centralblatt.—Leipzig, 1894. In progress.

Journal of the Royal Microscopical Society.—This contains a series of short reviews on recent microscopical work in all branches.

The Naturalist, London, publishes classified annual Bibliographies of Natural History for the North of England.

Annals of Scottish Natural History, Edinburgh, contains Bibliographies of current literature on Zoology and Botany of Scotland.

Wiegmann.—Archiv der Naturgeschichte. Yearly since 1835. Gives a record of zoological literature.

Engelmann, Bibliotheca Historico-Naturalis.—From earliest times until 1846; continued as Carus and Engelmann, Bibliotheca Zoologica, 1846–1860; continued as Taschenberg, Bibliotheca Zoologica, 1860–1880 (in progress). This contains a reference to biological literature, either separate or serial, exclusive of botany, and is by far the best and most convenient book ever published.

Agassiz, L.—Bibliographia Zoologiæ et Geologiæ. 4 vols. Ray Society. 1848–54. A general catalogue of all books, tracts, and memoirs on these subjects.

Index-Catalogue of the library of the Surgeon-General's Office, United States Army. 15 vols. Washington, 1880–1893. One of the most remarkable catalogues ever published—the papers are entered under both subject and author, and though essentially medical, it is of the greatest use in general biology.

Haller, A. von.—Bibliotheca Anatomica qua scriptorum ad anatomen et physiologium facientia a rerum initiis recensentur. Tiguri, 1774.

British Museum Catalogues. — In the recent volumes, *e.g.*, Birds, Marsupials, Snakes, Fishes, Fossil-Fishes, etc., reference is given, as far as practicable, to every described species, whether in the Museum or not.

Szinnyei, J., & J. Szinnyei.—Bibliotheca Hungarica historiæ naturalis et matheseos. Magyarorszag természettudomanyi és mathematikai könyvészete, 1472–1875. Budapest, 1878.

Krebel, R.—Russlands naturwissenschaftlichen und medicinischen Literatur. Die in *nicht* russischer Sprache erscheinen Schriften und Abhandlungen. Jena, 1847.

Koschenikov, G.—[Catalogue of Russian Zoological Literature. Vertebrata, 1885–1889.] 4to, 1893, 523 pp. [Not seen: in Russian.]

Camerano & Lessona.—Biblioteca della Zoologia e Anatomia comparata in Italia. Torino. Yearly, 1878–1880.

Monitore Zoologico Italiano.—Firenze. Yearly since 1890.

Cavanna, G.—Elementi per una Bibliografia Italiana intorno all' Idrofauna, etc. Firenze, 1880.

Kingsley, J. S.—List of American publications in Zoology for the current year. Amer. Naturalist.

Morris, J.—Catalogue of British Fossils. 2nd ed. 1854.

Bigsby, J. J.—Thesaurus Siluricus, 1868. List with references to all known forms of the Silurian Period.

Bigsby, J. J.—Thesaurus Devonico-Carboniferus. 1878. Similar to last, but Devonian and Carboniferous.

Etheridge, R.—Fossils of the British Islands Stratigraphically and Zoologically arranged. Vol. i. Palæozoic. 1888.

Bronn, H. G.—Index paläontologicus. 1848. All known fossil forms up to date of publication.

MILLER, S. A.—American Palæozoic Fossils. Ed. 2 (1877), ed. 3 under title "North American Geology and Palæontology." 1889. Also Supplements.

ETHERIDGE, R., jun.—A Catalogue of Australian Fossils (including Tasmania and the Island of Timor). Cambridge, 1878.

BRONN, H. G.—Klassen und Ordnungen des Their-Reichs. New ed. 1882–94. In progress. Gives a general bibliography to each group.

FOR NOMENCLATURE :—

SCUDDER.—Nomenclator zoologicus. 1885. This book includes Agassiz's Nomenclatores zoologici of 1846, 1848, and 1842–48; and Marschall's Nomenclator, 1878, and contains about 80,000 generic names.

[SHERBORN.—Index generum et specierum Animalium. In manuscript and in progress. Can be consulted, on application, at the Geological Library, British Museum (Natural History). References to all described recent and fossil forms.]

[INDEX ANIMALIUM.—The German Zoological Society are preparing a catalogue of all accepted recent species, with short diagnoses.] (See paragraph in this month's "News.")

VERTEBRATA.

GENERAL.

MINOT, C. S.—A Bibliography of Vertebrate Embryology. Mem. Boston Soc. Nat. Hist., vol. iv., no. 11, 1893.

WOODWARD & SHERBORN.—A Catalogue of British Fossil Vertebrata. 1890. With supplement in "Geological Magazine" for January, 1891. Gives full literature, synonymy, and localities for all known forms.

[Publications on special countries are not here given, but the reader will not omit to use Blanford's "Fauna of India," Grandidier's "Madagascar," Semper's "Philippinen," and similar works.]

MAMMALIA.

TROUESSART, E. L.—Catalogue des Mammifères vivants et fossiles. Bull. Soc. Sci. Angers. Appeared in parts between 1881–1884.

ROGER, O.—Verzeichniss bisher bekannter fossiler Saugthiere. Ber. Nat. Ver. Augsburg, 1887–94. Index to all known forms.

DOBSON, G. E.—Catalogue of the Chiroptera. 8vo, London (Brit. Mus.), 1878.

THOMAS, O.—Catalogue of the Marsupialia and Monotremata, etc. London (Brit. Mus.), 1888.

MASON, O. T.—Summary of Progress in Anthropology in 1890. Ann. Rep. Board Regents Smithsonian Inst., 1890 (1891), p. 527, etc. Yearly.

FLOWER & LYDEKKER.—A Manual of the Mammalia. 1891.

AVES.

WATERHOUSE, F. H.—Index Generum Avium. 1889.

GIEBEL.—Thesaurus ornithologiæ. Repertorium der gesammten ornithologischen Literatur. 1872.

SHARPE et alii.—Catalogue of Birds. 23 vols. In progress. London (Brit. Mus.) 1874, etc. Description with synonymy of all known forms.

FÜRBRINGER, M.—Untersuchungen zur morphologie und systematik der Vögeln. Amsterdam, 1890. Contains a general bibliography of bird literature dealing with anatomy and physiology recent and fossil.

COUES, E.—American Ornithological Bibliography. U. S. Geol. Survey Territories, Misc. Publ., no. xi, 1878; and Bulletin, vol. v., 1880.

REPTILIA.

BOULENGER, G. A.—Catalogue of Chelonians, Rhynchocephalians, and Crocodiles. London (Brit. Mus.), 1889.

BOULENGER, G. A.—Catalogue of Lizards. 3 vols. London (Brit. Mus.), 1885–87.

BOULENGER, G. A.—Catalogue of Snakes. Vol. 1. London (Brit. Mus.), 1893. In progress.

BOULENGER, G. A.—Catalogue of Batrachia. 2 vols. London (Brit. Mus.), 1882.

Pisces.

Günther, A.—An Introduction to the Study of Fishes. Edinburgh. 1880.

Günther, A.—Catalogue of Fishes. 8 vols. London (Brit. Mus.), 1859–1870. A new edition of the 1st volume is now in course of preparation by G. A. Boulenger.

Woodward, A. Smith.—Catalogue of Fossil Fishes. London (Brit. Mus.). In progress. This book mentions every described form; two volumes have appeared.

Agassiz, L.—Poissons Fossiles. 1833–43. In vol. 1 of this book Agassiz gave a bibliography of the subject.

Bosgoed.—Bibliotheca Ichthyologia et Piscatoria. 1874.

Westwood & Satchell.—Bibliotheca Piscatoria. (Angling.) 1883.

INVERTEBRATA.

General.

White & Nicholson.—Bibliography of North American Invertebrate Palæontology. Miscell. Publ. U.S. Geol. Surv. Territ., 1878, and Supplement, Bull. U.S. Geol. Surv. Territ., vol. v., 1879.

Thompson, D'Arcy W.—Bibliography of Protozoa, Sponges, Coelenterata and Worms, including also Polyzoa, Brachiopoda and Tunicata for the years 1861–1883, Cambridge, 1885.

Tunicata.

Herdman, W. A.—Tunicata. Report Voyage Challenger, vol. vi., 1882, and vol. xiv., 1886. Bibliography.

Crustacea.

Hoek, P. P. C.—Cirripedia. Report Voy. Challenger, vol. viii., 1883. Bibliography.

Stebbing, T. R. R.—Amphipoda. Report Voy. Challenger, vol. xxix., 1888. Complete analysis of all previous writings, occupying 600 pp.

Chilton, C.—The Subterranean Crustacea of New Zealand, etc. Trans. Linn. Soc. (Zool.), vol. vi., pt. 2, 1894. Bibliography.

Della Valle, A.—Gammarini del Golfo di Napoli. Fauna u. Flora G. Neapel, vol. xx., 1893. Bibliography.

Giesbrecht, W.—Pelagischen Copepoden. Fauna u. Flora G. Neapel, vol. xix., 1892. Bibliography.

Mayer, P.—Die Caprelliden des Golfes von Neapel. Fauna u. Flora G. Neapel, vol. vi., 1882. Bibliography.

Dohrn, A.—Die Pantopoden des Golfes von Neapel. Fauna u. Flora G. Neapel, vol. iii., 1881. Bibliography.

Edwards, H. Milne.—Histoire naturelle des Crustacées, 1834–1840. All known species up to 1840.

Vogdes, A.—Bibliography of Palæozoic Crustacea, 1698-1889. Bull. U.S. Geol. Survey, No. 63, 1890: second edition in Occas. Papers, California Acad. Sci., vol. iv., 1893. Bibliography and Index.

Arachnida and Myriapoda.

Walkenaer and Gervais.—Suites à Buffon—Aptères, 1837–1847. References to all known Arachnida, Myriapoda, and Aptera up to publication.

Simon, E.—Histoire naturelle des Araignées. 1892–93. All known genera of spiders characterised.

De Saussure and Humbert.—Études sur les Myriapodes. 1872. All known American forms.

Latzel, R.—Die Myriopoden, 1880–1884. Bibliography.

See Scudder, under Insects.

Insecta.

Hagen, E.—Bibliotheca Entomologica. 1862. Contains the literature of the group since the earliest times, since which date the Zoological Record constitutes the safest guide.

Keller.—Bibliografia Universali di Apicoltura. Milano, 1881.

Westwood, J. O.—Revisio insectorum familiæ Mantidarum. 1889.

Dalla Torre.—Catalogus Hymenopterorum. 1892. 11 vols., of which the first has appeared.

Kirchner.—Catalogus Hymenopterorum Europæ. 1867.

André E.—Species Hymenoptères d'Europe et d'Algérie. 3 vols. 1879-86.

Gemminger and Harold.—Catalogus Coleopterorum. 1868–1876.

Lacordaire & Chapuis. Genera des Coléoptères, etc. Paris, 1854–76.

Heyden, Reitter, and Weise.—Catalogus Coleopterorum Europæ, Caucasi, et Armeniæ rossicæ. 1891.

Chapuis & Candèze.—Catalogue des Larves des Coléoptères connues jusqu'à ce jour. Mém. Soc. Sci. Liège, viii., 1853.

Severin, L.—Catalogue général des Hemiptères. Bruxelles, 1893.

Schiner, J. R.—Fauna Austriaca (Diptera). Wien, 1862–64. All European species.

Schiner, J. R.—Catalogus systematicus Dipterorum Europæ. Wien, 1864. An index catalogue to the foregoing.

Osten-Sacken, C. R.—Catalogue of the described Diptera of North America. Smithsonian Miscell. Collection, no. 270, 1878.

Kirby, W. F.—A Synonymic Catalogue of Neuroptera Odonata. 1890. Recent and fossil.

Hagen, H.—Synopsis of the described Neuroptera of North America, with a list of the South American species. Smithsonian Miscell Coll., vol. iv., 1862.

Kirby, W. F.—A Synonymic Catalogue of Lepidoptera Heterocera. Vol. i., 1892. Sphinges and Bombyces.

Kirby, W. F.—A Synonymic Catalogue of Diurnal Lepidoptera. 1877. Bibliography and index.

Morris, J. G.—Synopsis of the described Lepidoptera of North America. Smithsonian Miscell. Coll., vol. iv., 1862.

Chambers, V. T.—Index to the described Tineina of the United States and Canada. Bull. U. S. Geol. Survey, iv., 1878.

Scudder, S. H.—Index to the known fossil insects of the world, including Myriapods and Arachnids. Bull. U.S. Geol. Survey, no. 71, 1891.

Scudder, S. H.—Bibliography of Fossil Insects. Bull. U.S. Geol. Survey, no. 69, 1890.

White, F. B.—Pelagic Hemiptera. Report Voy. Challenger, vol. vii., 1883. Bibliography.

Lichtenstein, J.—Les Pucerons. Monographie des Aphidiens. Montpellier, 1885. Bibliography and list of species known up to 1885.

Mollusca.

Paetel, F.—Catalog der Conchylien-Sammlung. Berlin, 1887–1890. This with few exceptions contains all shell-bearing mollusca known up to that date.

Pfeiffer, L.—Nomenclator Heliceorum viventium, etc. Cassellis, 1881. This volume contains all genera and species of non-operculate terrestrial molluscs known at that date, and includes references to Pfeiffer's Monographia Heliceorum viventium. 8 vols. 1847–1877.

Pfeiffer, L.—Monographia Pneumonopomorum viventium. Cassellis, 1852–1875. Includes all operculate molluscs known up to that date.

Cockerell, T. D. A.—A Check List of the Slugs. Conchologist, 1893, nos. 7 and 8.

Hermannsen, A. N.—Indicis generum malacozoorum primordia. 2 vols. 1846–1852.

Hoyle, E. W.—Cephalopoda. Report Voy. Challenger, xv., 1886. Refers to most of known forms.

Pelseneer.—Pteropoda. Report Voy. Challenger, xxiii., 1888. Refers to most of known forms.

Tryon, G.—Manual of Conchology, structural and systematic. In progress. All known forms figured and described.

Binney, W. G.—Bibliography of North American Conchology previous to the year 1860. Smithsonian Miscell. Coll. Vol. v., 1863, and vol. ix., 1869.

Dollfus and Ramond.—Bibliographie de la Conchyliologie du terrain Tertiaire Parisien. Paris, 1886.

Locard, A.—Prodrome de Malacologie Française. 8vo. Lyon, 1882–86. Bibliography.

Hudleston and Wilson.—Catalogue of British Jurassic Gasteropoda. 1892.

Gabb, W. M.—Synopsis of the Mollusca of the Cretaceous formation. 1861.

Brachiopoda.

Davidson, T.—Bibliography of the Brachiopoda. Palæontographical Society, 1886. [Mr. C. Schuchert is said to be preparing an index to all described American fossil forms.]

Bryozoa.

Busk, G.—Catalogue of Marine Polyzoa. 3 parts. London (Brit. Mus.), 1852–75.

Jelly, E. C.—Synonymic Catalogue of Recent Marine Bryozoa, including fossil synonyms. 1889.

Vermes.

Eisig, H.—Die Capitelliden. Fauna u. Flora G. Neapel, xvi., 1887. A book full of information of the most varied kind.

Lang, A.—Die Polycladen (See-planarien). Fauna u. Flora G. Neapel, vol. xi., 1884. Bibliography.

Grassi, B.—I Chetognathi. Fauna u. Flora G. Neapel, vol. v., 1883.

Schneider, A.—Monographie der Nematoden. Berlin, 1866. All species known up to that date.

Selenka, E.—Die Sipunculiden. In Semper's Reisen der Philippinen. Wiesbaden, 1883. Bibliography.

Linstow, O.—Compendium der Helminthologie. Hannover, 1878. Nachtrag, 1889. Bibliography.

Davaine, C.—Traité des Entozoaires et des maladies vermineuses, etc. Paris, 1887.

Rosa, D.—Revisione dei Lumbricidi. Mem. R. Accad. Sci. Torino, vol. xliii., 1893.

Moquin-Tandon, A.—Monographie de la famille des Hirudinées. Paris, 1846. Dr. R. Blanchard is, we understand, engaged on a new monograph.

Quatrefages, A. de. — Histoire naturelle des Annelés marins et d'eau douce. Paris, vols. i., ii., 1865; vol. iii., by Vaillant, 1889.

Vejdovsky, F. — System und morphologie der Oligochæten. Prag, 1884. Bibliography.

Graff, L. von.—Monographie der Turbellarien. I. Rhabdocœlida. Leipzig, 1882. Bibliography.

Hallez, P.—Catalogue des Turbellariés. Rev. Biol., v., 1893, pp. 186–197.

Parona, C.—Elmintologio Italiano, Bibliografio, Sistematico e Storio. Bolletino Scientifico, vols. xii., xiii., 1890–91.

Echinodermata.

Duncan, P. M.—A revision of the Genera and great groups of the Echinoidea. Journ. Linn. Soc. (Zool.), xxiii., 1889. The Pre-Linnean names must be avoided.

Agassiz, A.—Revision of the Echini. Illustrated Catalogue vii. of the Museum of Comparative Zoology at Harvard College. 4 pts. Cambridge, 1872-74. Bibliography, etc.

Wachsmuth, C., and Springer, F.—Revision of the Palæocrinoidea. Proc. Acad. Nat. Sci. Philadelphia, 1879, 1881, 1885, and 1886. Contains lists of all Palæozoic Crinoids described up to that date, with bibliographic references.

Carpenter, P. H.—Stalked Crinoids and Comatulidæ. Report Voy. Challenger, xi., 1884, and xxvii., 1888. Bibliography.

Etheridge and Carpenter.—Catalogue of Blastoidea in the British Museum. London (Brit. Mus.), 1886. Bibliography.

Barrande, J.—Cystidées. Systeme Silurien de Bohéme, vol. vii., 1887. Bibliography.

Lyman, T.—Ophiuroidea. Report Voy. Challenger, vol. v., 1882. List of all known forms.

Semper, C.—Holothurien. Reisen der Philippinen. Leipzig, 1868. Bibliography.

LAMPORT, K.—Die Seewalzen-Holothurioidea. In Semper's Reisen der Philippinen. Wiesbaden, 1885. Bibliography.

THÉEL, H.—Holothuroidea. Report Voy. Challenger, vol. xiv., 1886. Bibliography.

PERRIER, E.—Révision de la collection de Stellérides du Muséum d'Histoire naturelle de Paris. Arch. Zool. Expér., iv., 1875, pp. 1-185. Bibliography.

SLADEN, W. P.—Asteroidea. Report Voy. Challenger, vol. xxx., 1889, List of all known recent species.

COELENTERATA.

HAECKEL, E.—Siphonophora. Report Voy. Challenger, vol. xxviii., 1888. Bibliography.

HADDON, A. C.—Revision of the British Actiniæ. Sci. Trans. R. Dublin Soc., vol. iv., (1889, 1891, etc.). Bibliography.

ANDRES, A.—Le Attinie. Fauna u. Flora G. Neapel, vol. ix., 1884. Bibliography.

PORIFERA.

LENDENFELD, R. VON.—Monograph of the Horny Sponges. Royal Society, 1889. Includes a bibliography of all literature relating to the Spongida.

HINDE, G. J.—Catalogue of Fossil Sponges in the British Museum. London (Brit. Mus.), 1883. Bibliography.

VEJDOVSKY, F.—Die Süsswasser-Schwämme Böhmens. Abh. k. böhm. Ges., xii., 1885. Bibliography.

PROTOZOA.

SHERBORN, C. D.—A Bibliography of the Foraminifera. 1888. Includes all previous lists.

SHERBORN, C. D.—Index to the Genera and Species of the Foraminifera. Pt. I. A—Non. 1893. Up to December, 1889. Part 2 will appear in 1895.

WOODWARD, A.—The Bibliography of the Foraminifera, Recent and Fossil, including Eozoon and Receptaculites. 14th Ann. Rep. Geol. Nat. Hist. Survey Minnesota for year 1885.

LEIDY, J.—Fresh-water Rhizopods of North America. Rep. U.S. Geol. Surv. Territ., vol. xii., 1879. Bibliography of chief works on fresh-water Rhizopoda.

HAECKEL, E.—Radiolaria. Report Voy. Challenger, vol. xviii., 1887. Bibliography.

BRANDT, K.—Die Koloniebildenden Radiolarien (Sphærozoëen). Fauna u. Flora G. Neapel, vol. xiii., 1885. Bibliography.

CATALOGUES SHOWING WHERE TYPES CAN BE FOUND.

WOODWARD AND SHERBORN.—Catal. of British Fossil Vertebrata. London, 1891.

WOODS, H.—Catalogue of the Type Fossils in the Woodwardian Museum, Cambridge. Cambridge, 1891.

WILSON, E.—List of Fossil Types and described Specimens in the Bath Museum. Proc. Bath Antiq. Field Club, 1892.

WILSON, E.—Fossil Types in the Bristol Museum. Geol. Magazine, 1890.

CRANE, E.—Types and Figured Specimens now in the Brighton Museum. Report for 1891-92. Brighton, 1892.

PLATNAUER, H. M.—List of Figured Specimens in the York Museum. Ann. Rept. Yorkshire Phil. Soc. for 1890.

BOLTON, H.—Catalogue of the Type Fossils (Manchester Museum, Owens College). Manchester, 1893.

Report of a Committee upon the Provincial Museums of the United Kingdom. Rep. Brit. Assoc., 1887 (1888). Pp. 97–113. Gives Museum, curator, nature of collections, etc., etc.

MEDICINE.

NEALE'S Medical Digest; latest edition embraces references to all the contributions to some seventeen important English periodicals from 1840 to 1890; with elaborate index.

SCHMIDT'S Jahrbücher; references to and abstracts of the more important contributions to medical science throughout the world, from 1834.

Index-Catalogue of the Library of the Surgeon-General, U.S. Army. 1880-93. Catalogues and indexes the papers in nearly all the medical periodicals and reports in every language.

BILLINGS AND FLETCHER.—Index Medicus; begun in 1879, both author and subject index to all papers in any way bearing on medical science and surgery. Issued monthly. New York.

BOTANY.

GENERAL LITERATURE.

PRITZEL, G. A.—Thesaurus Literaturæ Botanicæ. Leipzig, 1872–74.

JACKSON, B. D.—Guide to the Literature of Botany, London, 1881.

HALLER, A. VON.—Bibliotheca Botanica, qua scriptorum ad Rem Herbariam facientia a rerum initiis recensentur. Tiguri, 1772.

SEGUIER, J. F.—Bibliotheca Botanica, sive Catalogus auctorum et librorum qui de Re Botanica, de Medicamentis ex Vegetabilibus paratis, de Re Rustica et de Horticultura tractant. Lugduni-Batavorum, 1760.

BOTANISCHES CENTRALBLATT (Cassel).—Yearly since 1880.

BOTANISCHES JAHRESBERICHT (Leipzig).—Yearly since 1874.

BRITTEN AND BOULGER.—Biographical Index of British and Irish Botanists. London, 1893. Gives brief notes on life, works, portraits, and collections.

FOR NOMENCLATURE:

PRITZEL.—Iconum botanicarum Index. 1861. Giving a reference to flowering plants and ferns discovered previously to 1860.

JACKSON AND HOOKER.—Index Kewensis plantarum phanerogamarum, etc. This book gives a reference to every described flowering plant from the time of Linnæus to the year 1885 inclusive. Two volumes A—Justicia are issued, the remaining two are promised shortly.

PHANEROGAMS.

DE CANDOLLE.—Prodromus Syst. Nat. Regni Vegetabilis. Paris, etc., 1824, etc.

DE CANDOLLE.—Monographiæ Phanerogamarum. 8 vols. Parisiis, 1878, etc. In progress. This amounts to a new edition of the earlier Prodromus.

BUEK, H. W.—Genera Species et Synonyma Candolleana, etc. 4 vols. Berlin and Hamburg, 1842–74.

NYMAN, C. F.—Conspectus Floræ Europæ. 8vo. Orebro, 1878–82.

BENTHAM AND HOOKER.—Genera plantarum, 1862–80.

DURAND, T.—Index generum Phanerogamarum, etc. Bruxellis, 1888.

CRYPTOGAMS.

TONI, J. B. DE—Sylloge Algarum omnium hucusque cognitarum. Patavii, 1889–94. Contains all the known forms of the algæ and the diatoms. In progress.

SACCARDO, P. A.—Sylloge Fungorum omnium hucusque cognitarum. 10 vols. Patavii, 1882–92. All Fungi known up to date.

JAEGER AND SAUERBECK.—Genera et Species Muscorum, etc. 2 vols. San Gallis, 1870–79. Came out in parts in Verh. St. Gallischen nat. Ges.

BOTTINI, A.—Bibliografia Briologica Italiana. Atti (Mem.) Soc. Toscana, xii. (1893), pp. 256–292.

HUE, A. M.—Lichenes exotici a Professore W. Nylander. Parisiis, 1892.

GOTTSCHE, C. M., et alii.—Synopsis Hepaticarum. Hamburgi, 1844–47. F. Stephani is busy upon the Hepatics, and his works should be consulted by the student.

SPRUCE, R.—Hepaticæ of the Amazon. London, 1885. Monographs several genera.

HABIRSHAW, F.—Catalogue of the Diatomaceæ. Ed. 2. By H. H. Chase. New York, 1885. Very few copies made by cyclostyle, but can be seen in the Botanical Department, British Museum (Nat. Hist.). Contains Bibliography.

GEOLOGY.

GENERAL LITERATURE.

THE GEOLOGICAL RECORD.—1874–1884. A yearly record of the geological literature of the world between these years. Discontinued. In vol. i., for 1880-1884, a complete list of all geological serials then existing is given.

Annuaire géologique.—Yearly since 1885. In progress. This book gives not only a list of the papers on geology published during the year, but also a careful digest of the contents.

Quarterly Journal of the Geological Society.—In November of each year is published "Additions to the Library." This gives not only separate books, but titles of papers in serial publications.

Dewalque.—Catalogue des ouvrages de Géologie, de Minéralogie et de Paléontologie, etc. Soc. géol. de Belg., 1884. Contains all works found in the principal libraries of Belgium.

Delesse et alii.—Revue de Géologie. Paris. Yearly. 1860–1876.

Cotta, B. von—Geologisches Repertorium. Leipzig, 1877. Literature of geology, including chemistry, mineralogy, and palæontology, from 1530–1876.

Verzeichniss der über Bergbau und Hüttenkunde, Salinenwasen, Mineralogie, Geognosie, und Geologie erschienenen Bücher, Karten und Zeichnungen. Eisleben, 1840–1852.

A Bibliography of Bibliographies of Geology is being compiled by Dr. E. de Margarie, and will be published in the Reports of the 1891 Geological Congress. This book, which may be expected shortly, will contain all the references here given, and many others which it is impossible for us to include.

SPECIAL GEOLOGY.

Africa.

Taramelli and Bellio.—Geografia e Geologia dell' Africa. 1890. Bibliography.

America.

United States Surveys.—A complete list of all publications of the U.S. Surveys was published by A. E. Foote in his Naturalists' Leisure Hour and Monthly Bulletin for August, 1886, while an additional list appeared in the same paper for October, 1890.

Marcou, J., and J. B.—Mapoteca Geologica Americana. 1752–1881. Bull. U.S. Geol. Survey, no. 7, 1884. A catalogue of geological maps of North and South America.

See all United States Surveys for literature of special areas.

Australia.

Etheridge and Jack.—Catalogue of works on the Geology, Mining, and Metallurgy, etc., of the Australian Continent and Tasmania. London, 1881.

Tate, R.—Anniversary Address. Trans. Adelaide Phil. Soc. for 1877–78. List of works on South Australian Geology.

Belgium.

Mourlon, M.—Géologie de la Belgique. 2 vols. Brussels, 1880–1. Bibliography.

Broeck, E. Vanden.—Esquisse Géologique . . . d'Anvers. Ann. Soc. Malac. Belg. Vol. ix., 1878. Bibliography.

Germany.

Renecke & Rosenbusch.—Einleitende Bemerkungen über die neue geologische Landes-Aufnahme von Elsass-Lothringen. Abh. Geol. Special-Karte Els.-Lothringen, vol. i., 1875. Bibliography of Geology of Alsace-Lorraine.

Jentzsch, A.—Die Geologische und Mineralogische Literatur des Königreiches Sachsen und der angrenzenden Landertheile im 1835 bis 1873, etc. Leipzig, 1874.

Eck, H.—Verzeichniss der Mineral. Geognost. Urgeschicht. und Balneogr. Literature im Baden, Württemberg und Hohenzollern, etc. Heidelberg (Mitth. Grossherz. Badischen geol. Landes.), 1890, 1891, and supplement 1893.

Cheluis, C.—Chronologische Ueberrsicht der geologischen und mineralogischen Literatur über das Grossherzogthum Hesse. Abh. Grossherz. Hesse geol. Landes., vol. i., 1884.

Kloos, J. H.—Repertorium der auf die Geologie, Mineralogie und Palæontologie des Herzogthums Braunschweig und der angrenzenden Landestheile bezüglichen Literatur; mit einer Karte; Braunschweig, 1892.

DEECKE, W.—Die Mineralogische, Geologische und Palæontologische Literatur ueber die Provinz Pommern. Mitth. nat. Ver. Neu-Pommern. xxv., pp. 54–92; 1894.

GREAT BRITAIN.

BLAKE, J. F.—Annals of British Geology. 1890, 1891, and 1892 have appeared.

SPECIAL BIBLIOGRAPHIES OF THE GEOLOGY OF THE COUNTIES OF ENGLAND AND WALES have been published by W. Whitaker and others as follows:—

BERKS (Rep. Brit. Assoc., 1882).
BUCKS (Rep. Brit. Assoc., 1882).
CAMBRIDGE (Mem. Geol. Survey: Expl. sheets 51 S.W. and 51 N.W., 1881).
CHESHIRE (Proc. Liverpool Geol. Soc., 1875–6).
CORNWALL (Journ. R. Inst. Cornwall, 1875).
CUMBERLAND (Trans. Cumberland Assoc., 1882).
DEVONSHIRE (Trans. Devon Assoc., 1880–84). See also for Literature of Kent's Hole, which includes many papers on early man (Trans. Devon Assoc., 1868, 1869, 1871, and 1884.
ESSEX (Essex Naturalist, 1889).
GLOUCESTER (see Somerset).
HAMPSHIRE (Journ. Winchester and Hampshire Sci. Soc. 1873). (See Weald.)
HERTS (Trans. Watford N. H. Soc., 1876).
ISLE OF WIGHT (Geol. Surv. Memoir: Geology of the Isle of Wight, 1889).
KENT (see Weald).
LANCASHIRE (Geol. Surv. Memoirs: Geology of the Burnley Coalfield, 1875.)
LANCASHIRE (Liverpool: Proc. Liverpool Geol. Soc., 1881).
MIDDLESEX (London) (Geol. Surv. Memoir: Geology of London Basin, 1872).
MONMOUTHSHIRE (see Wales).
NORFOLK (Geol. Surv. Memoir: Geology of the Country around Norfolk, 1881).
OXFORD (Rep. Brit. Assoc., 1882).
RUTLAND (Geol. Surv. Memoir: Geology of Rutland, 1875).
SHROPSHIRE (Trans. Shropshire Arch. Soc., 1889).
SOMERSET (Geol. Surv. Memoir: Geology of East Somerset, 1876).
SUFFOLK (Geol. Surv. Memoir: Geology of the Country round Ipswich, 1885).
STAFFORD (Rep. Brit. Assoc., 1885).
SURREY (see Weald).
SUSSEX (see Weald).
WALES (Rep. Brit. Assoc., 1880).
WARWICK (Rep. Brit. Assoc., 1885).
WEALD (Geol. Surv. Memoir: Geology of the Weald, 1875).
WESTMORELAND (Trans. Cumberland Assoc., 1882).
WILTS (Wiltshire Arch. Mag., xiv., 1873).
WORCESTER (Rep. Brit. Assoc., 1885).
YORKSHIRE (Davis & Lees, West Yorkshire, *London*, 1878).

For general literature of English Geology, the student cannot do better than refer to the footnotes in H. B. Woodward's Geology of England and Wales, 1887.

ICELAND & FARÖE.

TRIBOLET, M. F. DE.—Note sur les roches et minérales recueillis en Islande et aux îles Färöer. Bull. Soc. Sci. Nat. Neuchâtel, vol. xi., 1877. Bibliography.

INDIA.

OLDHAM, R. D.—A Bibliography of Indian Geology, etc., to the end of A.D. 1887. Calcutta, 1888.

ITALY.

BIBLIOGRAPHIE GÉOLOGIQUE ET PALÉONTOLOGIQUE DE L'ITALIE. Bologna, 1881 (2nd Geol. Congress).

BIBLIOGRAFIA GEOLOGICA ITALIANA. Yearly since 1886. R. Ufficio Geologico, Roma.

MALAGOLI, M.—Bibliografia geologica e paleontologia della Provincia di Modena. Ann. Soc. Nat. Modena, 1883.

PARONA, SACCO, and VIRGILIO.—Bibliografia Geologica del Piemonte. Boll. Soc. Geol. Ital., xii., 1894, pp. 825–882.

PANTANELLI, D.—Bibliografia geologica e paleontologica della Provincia di Siena. Cron. R. Liceo Guicciardini, 1877.

ACHIARDI, A. D'.—Bibliografia mineralogica, geologica e paleontologica della Toscana. Roma, 1875.

JOHNSTON-LAVIS.—The South Italian Volcanoes. Naples, 1891. Bibliography.

NETHERLAND INDIES.

VERBECK, R. D. M.—Literatuur von de geologie, mineralogie en den mijnbouw van Nederlandsch Oost-Indie. Jaarb. Mijnw. Ned. O.-Indie, vol. iv., 1875. Includes Java, Sumatra, Borneo, Banca, Billiton, Celebes, and surrounding islands.

RUSSIA.

NIKITIN, S.—Russkaya Gheologhicheskaya Biblioteka. Yearly since 1885. Gives all papers on Russian Geology, with abstracts in Russian and French.

BERG, E. VON.—Repertorium der Literatur über die Mineralogie, Geologie, Palæontologie, Berg- und Hüttenkunde Russlands bis zum Schlusse des xviii. Jahrhunderts. St. Petersburg, 1862.

SPAIN.

CASTRO, M. F. DE.—Noticia del Estado en que se hallan los trabajos del Mapa Geológico de España en 1° Julio de 1874. Bol. Comm. Map. Geol. Españ., vol. iii., 1876. Notes on the geological literature of Spain, Canaries, Cuba, Porto Rico, and Philippines.

MAFFEI AND RUA FIGUEROA.—Apuntes para una Biblioteca Española de libros, relativos al conocimiento y exploitacion de las riquezas minerales. Madrid, 1871–2.

CARON.—Bericht über eine Instructionsreise in Spanien. Zeitschr. Berg.-Hütt. Salinenw., vol. xxviii., 1880. Notes on geological literature.

SWITZERLAND.

FAVRE, A.—Revue Geologique Suisse, yearly 1869–92. In Eclogæ geol. Helvet.

MINERALOGY AND METALLURGY.

REICHARDT, G.—Bibliotheca rerum Metallicarum. Literature of mines and mining in Germany between 1856–1864. Eisleben, 1857-66.

BRITISH MUSEUM.—The Students' Index to Minerals. Several editions. A mere list of specimens exhibited, but of use for reference.

ENGLISH, G. L.—Catalogue of Minerals. Several editions. Philadelphia, 1890. A dealers' catalogue, and of much service for reference, as it gives, after the name of the mineral, its crystallographic form and chemical composition.

BESNARD, A. F.—Die Mineralogie in ihren neuesten Entdeckungen und Fortschritten im Jahre. Corr.-Blatt. zool. min. Ver. Regensburg, 1874 and on for several years.

MIERS, H. A.—Index to Mineralogical Papers. Yearly since 1884. Appears in the Mineralogical Magazine.

FOLKARD, H. T.—Wigan Library. Index Catalogue of Books and Papers relating to mining, metallurgy, and manufactures. Southport, 1880.

DANA, J. D.—The System of Mineralogy. Ed. 6. By E. S. Dana. London, 1892 Complete reference book to all known minerals.

LOCK, A. G.—Gold: its Occurrence and Extraction. London, 1882. Bibliography.

ROSENBUSCH, H.—Mikroskopische Physiographie der Mineralien und Gesteine. Vol. i. Die Petrographisch wichtigen Mineralien. Vol. ii. Massige Gesteine. Stuttgart, 1887. Ed. 2. 1892. Bibliography of each rock, and index to localities.

COHEN, E.—Zusammenstellung petrographischer Untersuchungsmethoden. Strassburg (privately printed), 1884.

HALLOCK, E. J.—Index to the Literature of Titanium. Ann. N. York Acad. Sci., vol. i., 1877.

Bolton, H. C.—Index to the Literature of Manganese. 1596–1874. Ann. Lyc. N. H. N. York, 1875.

Bolton, H. C.—A select Bibliography of Chemistry, 1492–1892. Smithsonian Misc. Coll. xxxvi., 1893. 1212 pp.

GEOGRAPHY.

Engelmann, W.—Bibliotheca Geographica. 1857–58. Contains all publications between 1750 and 1856.

Petherick, E. A.—Catalogue of the York Gate Library formed by S. W. Silver. Ed. 2. 1886. For books of travel and general literature of geography.

Petermanns Mittheilungen.—Yearly since 1855. Gives from time to time a list of all recent literature on geography and much on geology.

Smiley, C. W.—This author indexed the Lakes and Rivers of the United States with reference to Rand, McNally & Co.'s Atlases of 1881 and 1882. In the rivers the tributaries to the third or fourth degree are marked by indents. The pape appeared in Rep. U.S. Fish Commission, pt. x., for 1882. Washington, 1884.

Admiralty Catalogue of Charts, Maps, and Sailing Directions.—Especially useful to those wishing information on coast lines, depths of seas, etc.

It is impossible here to give all the special Bibliographies of countries: the reader should refer to the Catalogue of the Royal Geographical Society's Library, where most of these books can be found. (See paragraph on p. 158.)

REFERENCE BOOKS ON GENERAL SUBJECTS.

Orientalische Bibliographie.—1887. Yearly. Contains the title of every publication bearing upon or published in the East. Berlin.

Kukula, R.—Bibliographisches Jahrbuch der Deutschen Hochschulen. Innsbruck, 1892. Gives an account of German and Austrian Professors then living, with a list of their works.

Trübner's Catalogue of Dictionaries and Grammars.—Ed. 2. 1882.

Minerva; Jahrbuch der Gelehrten Welt.—(Trübner, Strassburg.) Gives staff of Universities and Institutions throughout the world.

Year Book of Scientific Societies.—For Great Britain only. Gives staff, nature and contents of publications.

Roper, F. S. C.—Catalogue of Works on the Microscope. 1865.

Deby, J.—A Bibliography of the Microscope and Microscopic Studies. 1882.

C. Davies Sherborn.

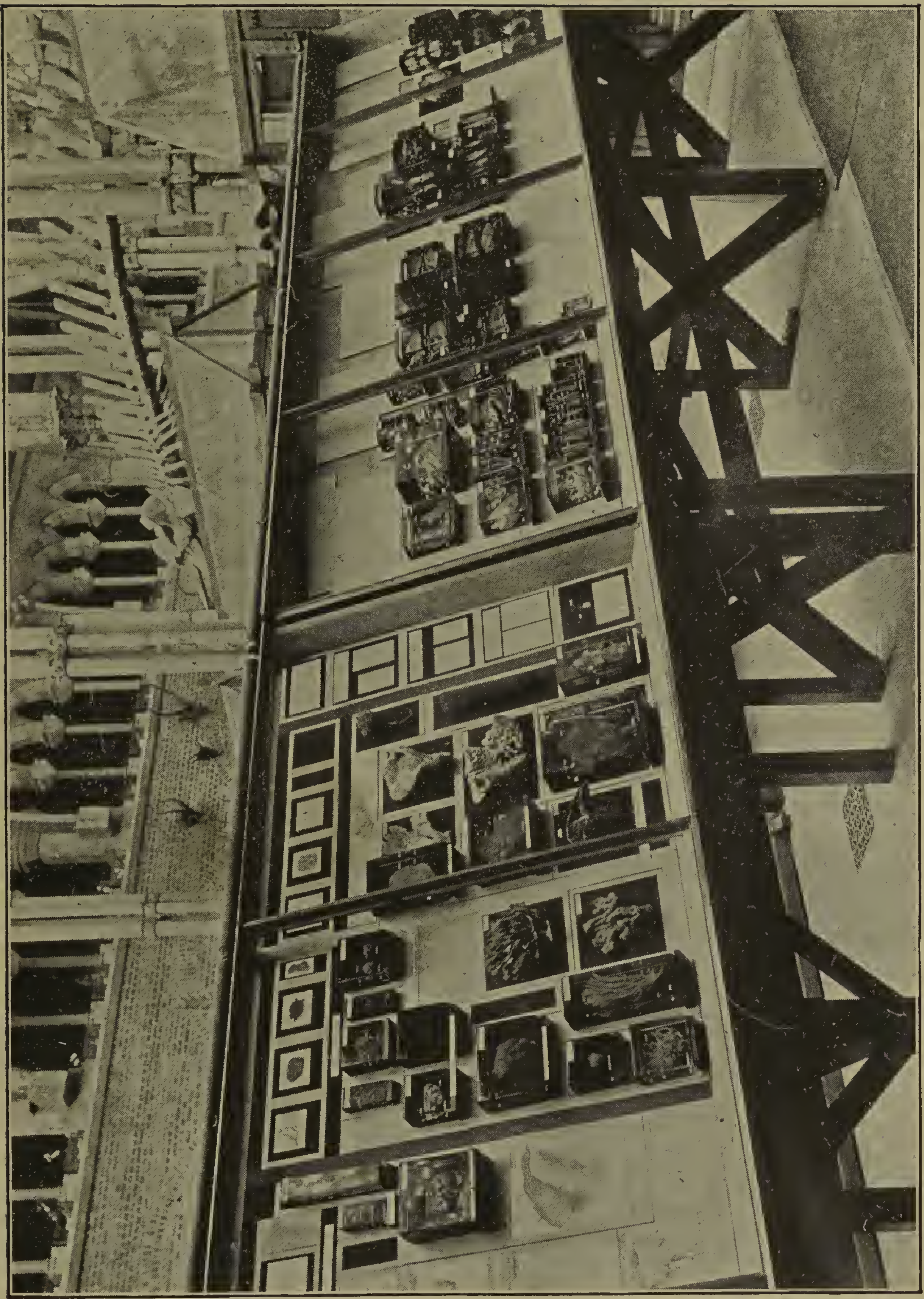
IV.

Some Reforms in the Oxford University Museum.

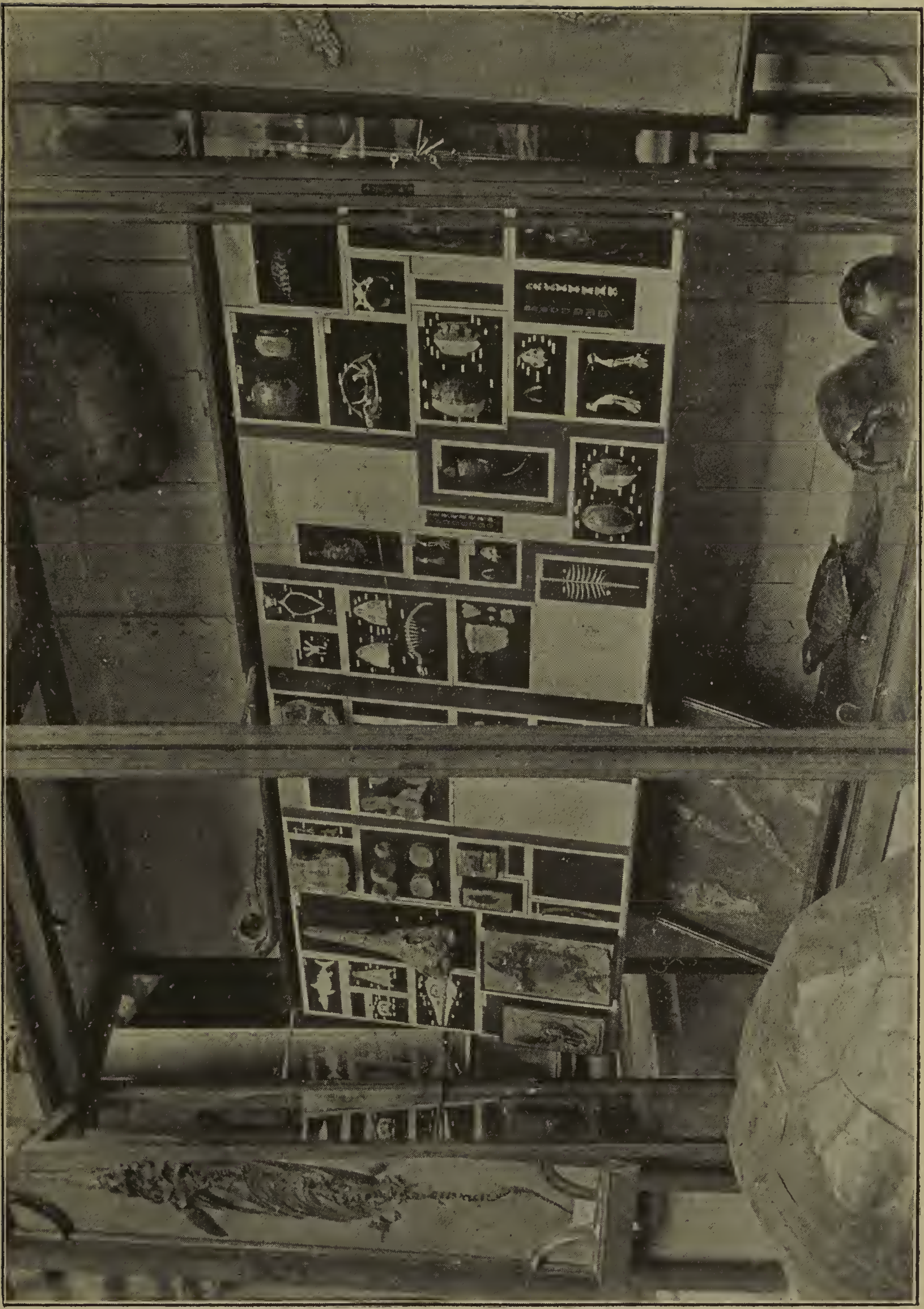
IT is now, I believe, some ten years since Sir William Flower inaugurated the reform of Museums on a truly scientific basis at the Natural History Museum; the cases in the Central Hall, gradually filling with excellent preparations, have been a valuable object lesson for all the scientific Museums in England. On somewhat similar lines, Professor Ray Lankester is reforming those exhibition cases containing zoological collections in the Oxford University Museum which are under his control.

So much has lately been said about the object and aim of Museums that I need only point out those particular conditions wherein this Museum differs from other similar public institutions as regards the kind of collections to be exhibited. Here one need seek neither to attract the nursery-maid nor to amuse children, nor again need one trouble to satisfy the idle curiosity of the sight-seer. There is, then, no necessity for tragic groups of stuffed animals, for birds perched on cardboard rocks among artificial flowers. On the contrary, the exhibits are to be strictly scientific, forming series at once instructive and interesting to the general educated public, and more especially to the real student of zoology. Surrounded as it is by the various chemical, physical, and biological laboratories, the central court is in the first instance a place of study. In such educational collections it is essential that each object should be exhibited for a definite purpose, should show what it is meant to show as clearly as possible, and should be fully labelled in language technical so far as is necessary for accuracy. The observer is not to be bewildered by a number of specimens, but rather impressed by a few well-chosen examples.

For more than 30 years a large and valuable quantity of material (chiefly vertebrates) has been accumulating in this Museum, which, enriched with portions of the Ashmolean, Christ Church, Hope, and other collections, forms a considerable store of specimens to draw upon for exhibition. Hitherto the cases belonging to the Department of Comparative Anatomy had been used rather for storage than for display. The specimens were placed on shelves,



One of the Invertebrate cases; showing, on the left-hand, the arrangement of the Porifera.



One of the Reptile cases; containing *Ichthyosauria*, *Plesiosauria*, and *Chelonia*.

and taken out when needed in the laboratories or elsewhere. A comparatively small number were shown in table cases. Now, two collections only are kept: one, the larger, for educational use in the laboratory and for research, the other, and smaller, for public exhibition. It is with the latter that we have to deal in the present sketch, and with the system adopted for its display. The specimens are placed in their proper systematic order to illustrate not only their structure from a general morphological point of view, but also those important characters of systematic value whereby animals are classified—in other words, not only to show the anatomy of animals and to state that they *are* divided into classes, orders, and families, but also to show *how* and *why* they are so divided. Extinct forms are placed next to their living relatives,[1] and treated in precisely the same manner, thus breaking down that artificial barrier some people are apt to erect between fossil and living animals. When neither specimens nor casts can be obtained, semi-diagrammatic black-and-white figures are set up in their place. Similar drawings are shown of important objects too small to be visible to the naked eye.

Each object is placed on a rectangular tablet covered with black in the centre, with a half-inch margin of soft greenish blue colour all round. The specimens are provided with printed labels, and narrow red "pointers," or lines; when in spirit, they are placed (labels and all), attached to a glass plate within square glass jars, blacked behind. Moreover, each tablet has a small label at the top right-hand corner, giving the scientific and popular name of the specimen, its date, catalogue number, locality, and source (by whom presented, or from whom obtained by purchase or exchange). As already stated, each specimen is placed in its systematic position, and the families or larger groups are marked off by dark red broad lines of separation. The whole presents a *tout ensemble* pleasing to the eye, yet not distracting the attention from what should, of course, be its chief object —the specimens themselves.

It is evident that the arrangement of the whole animal kingdom in this fashion is a task of no small magnitude, which will require some years to complete. Fortunately, in some instances, the help of specialists has been secured, as, for instance, of Dr. W. B. Benham for the Chætopods, Mr. E. A. Minchin for the Sponges, and Mr. G. C. Bourne for the Corals.

A set of six show cases, about twenty-one feet long, is devoted to the Invertebrates. These cases are provided with sloping backs and glass fronts on either side about four feet high above the ordinary table level, giving in all some 900 square feet of exhibition space (see Plate I.). Of the Invertebrates, a considerable number of Polychætes and Molluscs has been set up; but the Sponges are the only group the arrangement of which has approached anything like

[1] Professor Green has kindly lent many excellent fossils for this purpose.

completion. Mr. Minchin drew up the outline of the plan adopted, and selected the specimens. The Porifera, unfortunately, are a very unsatisfactory class to deal with in a Museum, their most important morphological and systematic characters being microscopical. This is, however, to some degree compensated by the very instructive and plainly visible modification of form assumed by individual sponges. A striking and very complete set of the modifications both of the Sponge Person and of the Sponge Colony has been prepared. Above these have been placed a series of coloured sketches drawn from life at Jersey, showing the brilliant colours of sponges which cannot be preserved. Then comes a series of fully described diagrams of the types of canal system, next to which are the specimens illustrating the classification of the group. (These do not appear in the photograph, being still in another case, where they were placed provisionally.) In this systematic part, short descriptions and figures of the characteristic spicules, etc., are placed at the head of each division.

Turning now to the Vertebrates. These are exhibited in the old high upright cases, within which sloping backs have been fitted. The Reptiles, the class to which most attention has been given, occupy one side of one of the main avenues, appropriately facing the famous *Megalosaurus* and *Cetiosaurus* remains. The tablet space devoted to this group alone is of about 240 square feet. Plate II. shows the case containing the Ichthyosauria, Plesiosauria, and part of the Chelonia. Below are put objects too bulky to be set on tablets, while above will be placed the outline classification of each order, with large and carefully-executed diagrams of the skull. The descriptive labels have not yet been written.

Besides this general collection, a few things of exceptional interest or rarity have been set up in special cases. Such are a series illustrating Craniometry, and another showing the principal points of structural difference or resemblance between Man and the Anthropoid Apes. Lastly, there is a case (Plate III.) containing the Mesozoic Mammalia from the Stonesfield Slate, and the remains of the famous Oxford Dodo, the last Dodo seen living in Europe. This case, which was the first of the new ones completed, is arranged in great detail. In the portion devoted to the Mesozoic Mammalia, we have on the left the Allotheria (Multituberculata), among which we may notice the long-lost fragment of *Stereognathus* belonging to Professor Lankester, which was only recently recovered. On the right are the six jaws from the Stonesfield Slate, with an enlarged drawing, history, and description below each, together with figures of all the British and the most important American genera of Mesozoic mammals. Alongside are placed specimens of those living forms which resemble them most. The largest portion of the case is occupied by the Dodo, the Solitaire, and their living allies. Most of the parts of *Didus* and *Pezophaps* are compared bone for bone with those of pigeons, and their affinities are fully illustrated.

Case containing the Mesozoic Mammalia and the Dodo.

In view of the approaching visit of the British Association to Oxford, it may be useful to mention some of the specimens, besides those already treated of, which are of particular interest to zoologists. Such are, among the mammals: The brain of the chimpanzee "Sally," the jaws of the Ziphioid whales, *Mesoplodon bidens* and *M. layardi*, and a cast of the large extinct American Ungulate, *Dinoceras mirabile;* a Great Auk's egg figures among the stuffed birds; among the reptiles we may mention the Leathery Turtle (*Dermochelys coriacea*), the structure of whose carapace is shown in the reptile case described above; among the interesting fish are *Lepidosiren* and *Chlamydoselachus*, *Palæospondylus* and *Pleuracanthus* of the fossil forms. In the Geological Department there is an excellent collection (that of the late Dr. Grindrod) containing type-specimens of *Pteraspis* and *Cephalaspis.* Among the recent invertebrates we may notice a large series of Cephalopods, some of which are unique in England (*e.g.*, *Thysanoteuthis* and *Dorataspis*); a specimen of the sponge *Cratero-morpha meyeri* containing the branching Polychæte, *Syllis ramosa*; a remarkable earthworm with two long penial appendages (of the genus *Siphonogaster*), and some very fine specimens of the coral *Heliopora.* Many dissections and other objects, though mounted, have not yet been set up in the new cases.

E. S. Goodrich.

Oxford Museum, July 14th.

V.

Hertwig's "Preformation or New Formation."[1]

Part I.

DR. OSCAR HERTWIG has worked so long at embryological subjects, and has produced results so valuable, that his opinion on this burning question of to-day is of unusual value. It is proposed in this paper to give an uncritical account of the views expressed by him in his most recent publication.

Biologists of the seventeenth and eighteenth centuries observed the process of embryological development as the growth of a miniature but visible embryo or bud into the adult form. They regarded it, therefore, as a process in which the minute particles of a formed organism became bigger by the absorption and transformation of food-substances. By analogical reasoning they carried these visible occurrences back to invisible beginnings, and came to hold the germ of an organism as a miniature image of the adult, invisible to us on account of the imperfection of our eyes, and by reason of the extreme minuteness and opacity of the germ. They had got beyond the idea of spontaneous generation, and had formulated a theory of the continuity of life in the phrases "omne vivum e vivo," and "omne vivum ex ovo." And so, in addition to the theory of "preformation," they adopted the theory of "evolution." The germ of an animal was a miniature of the animal, and within this, germ within germ, were enfolded the germs of all future descendants of the animal. The succession of the generations of animals was an "evolution," or unrolling of a series of germs arranged by the Creator at the beginning of the world.

In the last hundred years improvement of methods and instruments has carried the boundaries of the visible much further into matter. The embryos and buds which appeared then simple organic substance are now resolved into millions of cells, while these again are

[1] Zeit- und Streitfragen der Biologie. By Professor Dr. Oscar Hertwig. Pamphlet I. Präeformation oder Epigenese? Grundzüge einer Entwicklungstheorie der Organismen. Pp. 144, with 4 illustrations in the text. Gustav Fischer, 1894. Price 3 marks.

ordered aggregates of physical substances. Although in this minuter investigation no trace has been found of enfolding of germ within germ, yet as a logical possibility this theory could only fail when the actual limits of matter had been reached. It remains to-day as it was at origin, unfounded on observed fact, but in the limits of pure reason logically tenable.

Wolff's "Theoria Generationis" was the foundation of epigenetic views. He perceived that preformation and evolution formed a closed system where there was no room for the progress of research. He tried to bring back embryology from pure reason to the realms of observation, and held that the germs of animals and plants were structureless organic matter, which in the process of development by new formation or epigenesis became the organised adult. His theory found little fâvour in his own day, for he could offer no rounded interpretation of nature in which the mind of man could dwell. He had no explanation of that "vis essentialis," inherent energy, with which he endowed organic matter; on his system the growth of a new organism remained as much a miracle as he declared the theory of germ within germ to imply.

Since his time, succeeding generations have done something to place a real meaning within his inspiring but empty phrase, and in the slow progress of science step after step has been taken in tracing the forces and methods by which organic forms arise. But to-day, as formerly, the same question is answered in two ways. In the words of Roux: — "Is embryological development epigenesis or evolution? Is it a new formation of structure? or is it the becoming visible of structure previously invisible to us?"

Weismann is the great modern instance of those who respond for preformation and evolution. According to him, each cell or set of cells of the adult that vary independently have an actual representative in the germ. The germ is a microcosm in the strict sense of the word, and this microcosm is a miniature of the adult invisible to our eyes. In two respects, Weismann's germinal microcosm differs from that of the older evolutionists. They held it to be a simple and direct miniature of the adult. For us, separated from those times by a century of embryological work, the actual events of embryological development, the visible changes of cell-layers, and the metamorphoses of organs, make this view untenable. And so in Weismann's microcosm the elements representing structures in the adult are arranged quite differently from the adult arrangement. Secondly, the older evolutionists explained the chain of life by their theory of germ within germ. Weismann attributes to his microcosm the power of division. But in method and tendency this modern theory of preformation, like the older theory, stays rather than satisfies the desire for causal explanations, and is on the intellectual level of an "explanation" of the origin of the world by supernatural means.

For many years Dr. Oscar Hertwig has been studying both the

actual occurrences of embryology and their theoretical significance. Since he takes a more epigenetic view, he proposes to attempt to formulate his ideas. This he will do first in a criticism of Weismann's theory of preformation, and then in an exposition of his own epigenetic theory. An account of these two sections will appear in succeeding numbers of Natural Science.

P. Chalmers Mitchell.

SOME NEW BOOKS.

Marsupials.

A Handbook to the Marsupialia and Monotremata (Allen's Naturalists' Library). By R. Lydekker, F.R.S. 8vo. Pp. xvi., 302, with 38 coloured plates. London: W. H. Allen & Co., 1894. Price 6s.

In this volume the author gives a concise account of all the known species of the Marsupialia and Monotremata. Under each specific name we find (1) a list of synonyms, (2) a diagnosis of the species, (3) distribution, (4) an account of the habits and mode of life. Moreover, each family and genus is the subject of a short descriptive account, and the whole is prefaced by a general introduction. The design of the book, which is based avowedly upon Mr. O. Thomas's Catalogue of the Marsupialia and Monotremata in the British Museum, is, indeed, excellent, and renders reference to any point easy. There are both systematic and alphabetical indexes.

In the introduction the author remarks that "it is not to the credit of the present generation that the working zoologist has for the most part to rely for his knowledge of the habits of the greater number of Marsupials upon observations—admirable in their way—published many years ago"; but in spite of this deficiency most readers will be thankful for the very interesting accounts here brought together of the habits of the various species, and we may echo the hope of the author that the appearance of this book may stimulate those who have the opportunities to add to our knowledge in this direction.

The book is thoroughly up to date, and includes descriptions of the more recently discovered species. The most interesting of these is the curious Marsupial Mole (*Notoryctes typhlops*)[1], of which a complete description and a good figure are now rendered easily accessible to the general reader. The affinities of this remarkable animal have been the subject of some discussion, but it is now definitely proved to be a true marsupial, having, however, some striking points of resemblance with the Cape Golden Moles (*Chrysochloris*), which belong to the Insectivora. This similarity is particularly marked in the case of the teeth, and in this connection the author makes a statement that appears open to objection. He remarks that "the exact similarity between the molar teeth of the two is somewhat difficult to explain, although it may probably be accounted for by both having retained this primitive type of tooth from early ancestors." Now, it seems highly improbable, to say the least, that two animals belonging to two totally different groups, both highly specialised in relation to a peculiar mode of life, should have retained the primitive character of the teeth, which so readily undergo modification. It appears far more likely that in these cases, as in many others, the tritubercular teeth are by

[1] *See* Natural Science, vol. i., pp. 37 and 247.

no means primitive, but highly specialised in relation to a particular diet, and that the resemblances between the two animals in question are simply the result of convergence, due to similar environment and mode of life.

The last part of the volume is occupied by a brief account of the fossil forms of the two orders, concerning which Mr. Lydekker can speak with authority. It is to be hoped that a similar addition may be made to the other volumes; the divorce between descriptions of recent and fossil forms has lasted too long already.

Most of the plates were originally published in Jardine's Naturalists' Library, but have been recoloured for the present issue, while figures of some of the recently discovered species are added.

British Passerines.

A Handbook to the Birds of Great Britain. Allen's Naturalists' Library. By R. Bowdler Sharpe, LL.D. Vol. i. 8vo. Pp. xix., 342, with coloured plates. London: W. H. Allen & Co., 1894. Price 6s.

The additional interest which nowadays attaches to most natural history topics has naturally resulted in a rapid multiplication of zoological works. This in itself is desirable enough, since students always wish to keep their knowledge abreast of the times. The misfortune is that some of the works which are most loudly trumpeted by the public Press possess little real utility, being, in fact, merely popular compilations of the harvesting of genuine workers in the wide fields of Science. Happily, no such drawback attaches to the charming little volume on British Passerine Birds, designed by Dr. Sharpe to inaugurate the new "Naturalists' Library" which Messrs. Allen propose to launch under his very competent editorship. It is true that books on British birds are legion; nevertheless, there is always room for first-class work, and Dr. Sharpe's exceptional experience stamps his pages with that *imprimatur* of authority which we should look for in vain in many other popular works. Dr. Sharpe has the resources of the national collection upon which to base his conclusions. He is a good field naturalist himself, and possesses a knowledge of modern ornithological literature second to none. It is not, therefore, surprising that his new book has been carefully planned and executed with the highest skill. It is well printed, and misprints are hardly to be found. There are a few trifling slips in Dr. Sharpe's definitions of the ranges of certain birds, but none of these happen to be of any consequence. A more serious drawback is the somewhat inadequate treatment meted out to *nestling* birds. In this respect alone Dr. Sharpe is arbitrary and irregular. For example, he describes the nest-dress of the young Creeper (*Certhia*), but he says nothing about the first plumage of the true Flycatchers (*Musicapa*). The first plumage of the Thrushes (Turdidæ) is fully explained, but nothing at all is said about the first plumage of the Larks (Alaudidæ), which is quite as noteworthy. We should like to see the nest-dress of every species fully explained in the volumes which are to follow. When we turn to Dr. Sharpe's descriptions of the habits of birds, we find such frequent references to Mr. H. Seebohm that a stranger might suppose that this distinguished traveller was our only insular field naturalist. We do not wish to detract from the merits of Mr. Seebohm's labours, which we value very highly; but Mr. Seebohm's field notes have been served up again and again in his "History of British Birds," in his books on Siberia so-called, in the Ibis, in Dresser's "Birds of Europe," while popular com-

pilers have appropriated whole pages of Seebohm's writings in order to use them as padding in their own books. Dr. Sharpe would have done well to lighten his text with a brief extract from Warde Fowler's excellent *brochure* on the habits of the Marsh Warbler, or Gurney's graphic account of the Bearded Titmice that nest (in sadly diminished numbers) in the Norfolk Broads. A long series of excellent papers might have been drawn upon with advantage. After all, we must not be too exacting. It is something of a privilege that Dr. Sharpe should lay aside his important monographs in order to instruct our youth in such a limited subject as "British Birds," and we might seem ungrateful if we found serious fault with the execution of his task. It is only fair to say that Dr. Sharpe has taken great trouble to anticipate the requirements of beginners, as well as the points that advanced students are likely to raise. The characters of each genus are tersely and comprehensively described. The various problems of distribution are discussed and explained with judgment. Every variety of information seems to be afforded by one or other of the separate paragraphs into which Dr. Sharpe has wisely divided his text. The changes of plumage receive full and lucid treatment. The size and colouration of the egg-shells of British Birds are fully described, apparently from the National Collection, which Mr. Seebohm has recently arranged and classified. Altogether, we may say that Dr. Sharpe has been exceedingly successful in his first venture, which augurs well for the quality of the numerous volumes which are to follow. The most grudging critic would find it hard to withhold a high meed of praise from the letter-press. From beginning to end it is nothing less than admirable. Would that it were possible to speak in the same terms of the coloured plates. They are numerous—surprisingly so for the price of the book—and we hope that they may prove useful; but they are not up to the mark by any means. If the series arranged by Messrs. Allen is intended to have a large circulation, we incline to think that the publishers would do well to sanction a more liberal expenditure on illustrations.

American Eggs.

Smithsonian Institution. Comparative Oology of North American Birds. By R. W. Shufeldt. 8vo. Pp. 461–493. Washington, 1894.

Dr. Shufeldt is always interesting, and every one of his numerous papers possesses the value which independent and original views are sure to command. The Comparative Oology of North American birds is an attractive subject, far too comprehensive, in fact, to be satisfactorily dealt with in a pamphlet of only thirty octavo pages. The most prominent defect in Dr. Shufeldt's essay is that he has given his facts in so condensed a form, and in such intricate sentences, that it is often difficult to follow his arguments or even to comprehend the conclusion at which we are expected to arrive. The chief portion of the information here cited is borrowed direct from well-known sources, notably, from the writings of Messrs. Coues and Ridgway, which have long been accessible to everyone at home and abroad. Occasionally Dr. Shufeldt ventures upon statements which are certainly wide of the fact: as, for example, when he asserts that the American buzzards "never defend their eggs by direct attack." Indeed it would be easy to enumerate a number of points upon which Dr. Shufeldt's opinion should be modified by the experience of his American confrères. But there is much that is good

and accurate in his text; not that we can agree with his dictum that, "from a scientific standpoint, avian oology has accomplished much in the past," for the study of egg-shells has always appeared to us to be singularly barren of important results, however pleasurable it may be to its devotees. The biological laws, which Dr. Shufeldt formulates to explain variations in the colour of the shells of eggs, seem to us to be very unsatisfactory where they are not the exposition of self-evident truths. The fact of the matter is that Dr. Shufeldt has attempted to perform an impossibility, in compressing so large a subject into such narrow limits. That he has dealt with it at all is a matter for congratulation. Cursory and superficial as his review of American oology must be admitted to be, we feel grateful that he has endeavoured to generalise at all on this difficult theme. It is to be hoped that he may find leisure to develop his oological impressions; in which case we shall, no doubt, welcome a substantial addition to the literature of the subject at some future date.

H. A. MACPHERSON.

LINNÆUS'S "SYSTEM OF NATURE."

CAROLI LINNÆI SYSTEMA NATURÆ. Regnum Animale. Editio decima, 1758, cura Societatis Zoologicæ Germanicæ iterum edita. MDCCCXCIV. Lipsiæ sumptibus Giulielmi Engelmann, 1894. Pp. iv., 824. Price 10 marks.

As the Bible is to the theologian, so is the tenth edition of Linnæus's "Systema" to the systematic zoologist.

The foundation of all binomial nomenclature, the whole scientific naming of existing animals, dates from this tenth edition of Linnæus's "Systema." In the tenth edition for the first time, the immortal naturalist was consistent in giving two names, a genus-name and a species-name, to everything he described, and therefore the tenth edition represents the sum-total of the Linnæan labours as regards founding a definite and simplified system of labelling organic nature.

For many years, and chiefly at the instance of the Committee formed by the British Association in 1842, the twelfth edition of the "Systema" was taken as the base-line, but the tenth is slowly replacing the twelfth in this country,—it has long done so in America,—and this replacement smooths over not a few difficulties that exist from works published between the dates of the two editions (1758 and 1766).

The original edition is now so scarce as to command a high price, and the thanks of all students of zoology are due to the German Zoological Society, and to Mr. Engelmann, for the publication of this reprint, at a price which brings it within easy reach of anyone who takes the smallest interest in his subject. We specially recommend it to the notice of all museum curators and librarians.

SEX.

UEBER DAS VERHÄLTNISS DES MANNLICHEN UND WEIBLICHEN GESCHLECHTS IN DER NATUR. By Dr. Georg Klebs, Professor of Botany in Basel. Pp. 30. Jena: Gustaf Fischer, 1894. Price m. 0·80.

THIS little pamphlet does not call for much comment. It was delivered as the Rectorial Address in the University of Basel last autumn, and for publication, says the author, has been altered and enlarged in a few points. It forms a readable and quite uncontroversial account of the general phenomena of sex in the animal and vegetable kingdoms. The language is simple, and—rare occurrence

in modern scientific pamphlets—the references to all the writers mentioned are pleasant and appreciative. Starting with the discovery, in August, 1677, of spermatozoa by Leeuwenhoek's pupil, Hamm, the writer traces the gradual advances in knowledge which have led to our modern conception of the sexual cells in animals and plants. He shows the general parallelism existing between animals and plants in respect both of the sexual cells and of the male and female organisms containing them. He shows how, in both, the male cells become adapted to reach the usually larger and more passive female cells, and how the sexual characters of the male and female organisms are adapted in association with these diverging capacities. He touches lightly upon the determination of sex, the influence of crossing, and of in-and-in breeding and kindred topics. We recommend this book readily—not to biological specialists, who would find in it nothing very new, but to anyone who wishes an agreeable and instructive hour's reading.

Biological Homilies.

Biological Lectures and Addresses delivered by the late Arthur Milnes Marshall. Edited by C. F. Marshall. 8vo. Pp. viii. and 363. London: David Nutt, 1894. Price 6s.

"Homilies," says the Judicious Hooker, "are plain and popular instructions." Justly, then, may we apply this term to the essays comprised in the present volume. The exposition of the advanced teachings of modern biology in language that shall be lucid yet not misleading, intelligible yet not inaccurate, increases in difficulty with the increase and extension of knowledge itself. Along whatever path of zoological specialisation we proceed, we are speedily confronted by a bristling array of technical terms, erected as necessary waymarks for the pioneers themselves, but presenting a well-nigh impenetrable barrier to their merely curious followers, while they are also unintelligible to their fellow workers on other paths. We, who month by month endeavour to lay the results of special research in an intelligible form before our readers, well understand the difficulties of such a task, and readily appreciate how successfully they have been overcome by the enthusiastic biologist whose loss we still so deeply mourn.

Most of the lectures contained in this book are reprinted from the *Transactions of the Manchester Microscopical Society* or from other publications. Five, however, are now printed for the first time from such manuscripts as were left in a fit state. These five were mostly delivered thirteen years ago, and include "The Modern Study of Zoology," "The Influence of Environment on the Structure and Habits of Animals," "Embryology as an Aid to Anatomy," "The Theory of Change of Function," and "Butterflies." The remainder deal chiefly with Cell-division, the theories of Weismann, experimental Embryology, and above all with the Recapitulation Theory, which indeed is a regular King Charles's head.

The merit of these addresses is to be found rather in their clear and vivid style than in any great originality of idea. Perhaps the earlier pages have occasionally too strong a platform flavour, which the author himself would probably have edited out. In other respects the volume contains admirable models for the populariser of biology, refreshing reading for the amateur scientist, and reliable synopses for those whose special studies have not left them time to follow the intricacies of other branches than their own.

THE BRACHIOPODIST'S VADE-MECUM.

AN INTRODUCTION TO THE STUDY OF THE BRACHIOPODA, intended as a Handbook for the use of Students. By James Hall, assisted by John M. Clarke. The First Part, issued in *Report of the Regents of the N.Y. State Museum*, xlv., for 1891, pp. 450-616. Albany, dated 1892, but published in 1894.

IN our first volume (pp. 628–629) we noticed the important work by Messrs. Hall and Clarke, entitled "An Introduction to the Study of the Genera of Palæozoic Brachiopoda." The present handbook is practically an attempt to bring the facts and conclusions of that advanced treatise into a form more suited to the pecuniary and intellectual capabilities of the student. Hitherto English-speaking students have either had to struggle with the ponderous tomes of Davidson, issued by the Palæontographical Society, or, if they wished for some more concise, elementary, and, at the same time, up-to-date account, they have had to overcome the difficulties presented by a foreign language, and to rely on the admirable summaries by Von Zittel in German, and by Oehlert in French. Although, therefore, Professor Hall tells us that "the work has been prepared for the use of American students," we take upon ourselves to assure him that, so soon as it is issued in completed book form, it will win the gratitude of all those English-speaking zoologists and geologists who share with the authors an interest in the Brachiopoda.

The present part begins with a general account of the group, which follows in the main the lucid model furnished by Oehlert in Fischer's Manuel de Conchyliologie, differing therefrom by the incorporation of the fresh knowledge acquired during the last seven years. After a synopsis of the habits of the Brachiopoda and of their bathymetric and geographical distribution, the latter illustrated by a detailed map, there follows a full description of the shell with its external and internal markings. A closer attention to the facts of growth has recently led zoologists to distinguish between the various methods in which the passage for the stalk or peduncle has become more or less closed by shelly matter or stereom in different genera of Brachiopoda. Those who wish to learn the meanings of the terms *delthyrium*, *deltidium*, *deltidial plates*, *chilidium*, *listrium*, and *syrinx*, will here, and here only, find them fully explained. Next to this comes a clear and succinct account of the anatomy of the soft parts of the animal and of those internal skeletal structures connected with the so-called "arms" or lophophore that are of such importance in classification but so hard to investigate and comprehend. We find herein full use made of the recent researches of Joubin, Oehlert, Beecher, and others, of which accounts have from time to time been given in NATURAL SCIENCE, while, in the description of the development, ample notice is taken of the growth of the shell as well as of that of the soft parts, and it is recognised that developmental change does not cease at the close of embryonic life, but continues through the brephic, neanic, and succeeding stages.

With the way thus cleared for him, the student passes next to the study of all the genera, both recent and fossil, beginning, of course, with the Inarticulate Brachiopods, and then advancing through the Orthoid, Strophomenoid, and Productoid groups of the Articulata. The order that is followed in the descriptions of the genera is precisely the same as that of the larger "Introduction" to which we have already referred. The only differences appear to be the admission of Beecher's *Paterina*, which for some reason was excluded

from the previous work, and of Matthew's *Trematobolus*, which dates from 1893. This part of the work is illustrated by numerous woodcuts; at the same time it will undoubtedly be necessary for those who wish to gain a clear idea of the various genera, now so minutely divided, to refer to the excellent plates of the larger work, and better still to such specimens as they may be fortunate enough to find showing the details of deltidial, muscular, and brachidial structure, which specimens, we need hardly say, are rarely to be found even in our best appointed museums. Owing to the great difficulty of determining from ordinary specimens to which of these revised genera a species should be referred, we cannot help regretting that the authors have limited themselves under each genus to instancing merely the type species, which of course is not necessarily a typical or common species, instead of giving a list of the chief American and European species that they would refer to the genus in question. We may add, also, that considerable use of the larger work has brought to light a defect, which is naturally intensified in this smaller book, namely, a want of clearness and definiteness in pointing out the characters that separate one genus from another closely allied to it. It is, for instance, very difficult, in many cases, to know whether to refer a species to *Rafinesquina*, *Stropheodonta*, or *Orthothetes*. The diagnostic characters are no doubt given, but they are mixed up with other characters not strictly diagnostic. This defect might easily be remedied by giving a key, like that published a short time ago by Mr. C. Schuchert, and we trust that some such step will be taken in the second part of the present book.

As in their previous work, the authors do not divide their genera into families, a course which may be warranted by the insufficiency of our knowledge, but which hardly commends itself to the text-book compiler, the museum curator, or the examinee. We all admit that our classifications probably do not tally with nature; but even a bad classification is better than none. The authors, however, promise to discuss the question of classification at the close of the work, and meanwhile we may refer to the very workable classifications by Schuchert (*American Geologist*, xi., p. 141, and xiii., p. 102). We are more inclined to quarrel with our authors for the arrangement that they have in some instances adopted. They need not commit themselves to a cast-iron classification, but surely they might follow an arrangement more in accordance with the facts of geological history and individual development. They appear to accept the main arguments of Beecher, but for some unexplained reason they do not admit the natural conclusions. Thus, they begin with the somewhat advanced Lingulidæ, then proceed through the Lingulellidæ and *Lingulasma* to the Trimerellidæ, from which they come back to the Obolidæ, and so to *Paterina*. Now if they accept Beecher's views on development, they should also accept his conclusion that *Paterina* is the most primitive form of Brachiopod known to us, and they should lead off with that genus. But perhaps this will be explained in their final chapter. Anyhow we know exactly what Messrs. Hall and Clarke themselves think about the Trimerellidæ, and we know that they derive this assemblage from two distinct stocks, one typified by *Lingulasma* and the other by *Obolus*, a view which may be correct, but which is at any rate incompatible with the peculiar arrangement they adopt. This, however, will not render their book of less practical use to the student, who will look forward with much anxiety to the second part, which we are promised in the Annual Report of the State Geologist for 1893, and which we may, therefore, expect to receive somewhere in 1895 or 1896.

When the work is complete it is sincerely to be hoped that it will be bound up together and placed on the open market.

F. A. B.

Another Text-Book of Zoology.

Lehrbuch der Zoologie fur Studirende und Lehrer. By Dr. J. E. V. Boas. Lector der Zoologie an der Kgl. Landw. Hochscule Kopenhagen. Pp. 503, 427 illustrations. Second and enlarged edition. Jena: Gustaf Fischer, 1894. Price 10 marks.

In the increasing multitude of foreign and English text-books, it is not always easy to see any but local pretext for the issue of the books. It is natural and right that where a teacher is honoured in the country in which he teaches, his students should have his written as well as his spoken word. This text-book is written specially for medical students, students of veterinary science, and so forth. Those of us who have taught or examined similar students in England will wonder with a great admiration at the range and thoroughness of the course of zoology mapped out for these in Copenhagen. In a general part of eighty-eight pages Dr. Boas gives an account of the animal body on the old Hunterian plan of division into tissues and systems. Then he treats rapidly but briefly the special problem of Morphology, the plans on which animal organisms are built, the embryological growth of organisms, and the relations of the diverging types of adult structure to each other. Next he treats of various biological problems, such as the influence of sessile life, the duration of life, adaptations, the relations of animals to their environment, and so forth. All this is as simply and as philosophically done as we have seen in any book, and Dr. Boas's students are to be congratulated on their teacher. The systematic part is equally well written. In invertebrates the most typical or the best known species only are referred to. In vertebrates the enumeration of species is carried out much more fully.

The illustrations are all good. There are many old friends and many desirable additions. Altogether we have nothing but praise for this text-book. While it is not conspicuously better than the other leading books of the same kind, it is thoroughly satisfactory for its purpose, and much better than any English text-book or translation.

A Geological Text-book.

Geology. A Manual for Students in Advanced Classes and for General Readers. By Charles Bird, B.A. Lond., F.G.S. 8vo. Pp. 429. London: Longmans, 1894. Price 4s. 6d.

The illustrations constitute the feature in which this book differs most markedly from most works of the kind. These fall into three groups. First, and most important, a new set processed from photographs, some from those by Messrs. Wilson, chiefly of coast scenery, mountains, rivers, and old volcanic tracts, which are admirable in selection and in method of reproduction; the rest, of rocks, minerals, and fossils in the Jermyn Street Museum, often of figured or type-specimens, among which special attention should be given to figs. 145, 214, 231, and 237; both of these sets of illustrations are excellent and will give more than a passing value to the pages they adorn. Secondly, there are illustrations, for the most part good ones, borrowed from earlier works, some with adequate acknowledgment, as in the case of those from the works of Green and Bauerman, others without, like figs. 170, 211, 239, 259, 261, 287, which appeared first in Woodward's "Geology of England and Wales." Thirdly, those which have been recovered from some forgotten talus heap, or

else specially drawn for the work, as we do not recollect having seen them before, and fervently desire never to see them again. For example, the drawing of the quartz crystals on page 13 is preposterous, of hornblende on page 30 useless, while the fancy glacier on page 115 has no excuse, when good photographs, which might really teach something, are so easily obtainable. Why should a ridiculous and imaginary creature be made to masquerade as a trilobite on page 194, when a very good figure of *Ogygia Buchii* is wasting away in the uncongenial air of the Cambrian a few pages further on? Impossible graptolites, undesirable plesiosaurs, and shells and sponges standing on their heads should not have been tolerated amongst the other illustrations, and more satisfactory photographs or drawings of some of the rock-specimens should have been secured. It is tantalising not to be told where a conformable sequence from Carboniferous to Keuper is to be found (p. 273), or where the Lias rests direct in the Permian conformably and unconformably within a short distance, while the Oolite is unconformable to the Lias (p. 284).

Leaving these and other errors in the illustrations and turning to the text, it is clear that this has suffered by being cast in the rigid mould of a South Kensington Syllabus, so that an attempt has to be made to put into the book far more than can be *taught*, though it may possibly be *crammed*, in a work of this size. What is the use of attempting to teach anything about systematic crystallography without at least a drawing to show the relations between crystal axes and faces, or to simply state. without any explanation, that a crystal has or has not an effect on polarised light, unless these statements are merely intended to be committed to memory? The petrology is worse than the mineralogy; after learning that mere disintegration will convert granite into "gravel," that phonolite is "acidic," syenite basic, felsite and pitchstone plutonic, we are not much surprised to hear that the last class of rocks is often amygdaloidal. Slips and misprints abound: Marlsto, Brokenhurst, labrodorite, *Hippurites ponderosum*, Flanborough, Coraline, kim-clay, *Unio* (p. 320), Landeilo, Geographical Survey, *equivalvus*, while some of the definitions, doubtless accurate, do not help very much in understanding the application of words, like asbestos, unquenchable, and eggstone for roestone.

The latter part of the book is rather better done, the physical part decidedly so, and among the good features are vertical sections of the systems, showing their characters, fossils, and economic products. There are disfigurements, however, here and there. We read of trilobites with only two segments, that there is a Rhætic fish still living, that the "characteristic ammonites occasionally show a tendency to become a little mixed," and that belemnites are the "internal shells of a kind of cuttlefish." It is a pity the author has not cleared things up for the student by adopting the tripartite division of the older Palæozoic rocks now everywhere adopted, and that he has omitted all reference to the Ordovician and Silurian rocks of the Lake country, while giving a careful description of the foreign equivalents. Scant space is devoted to modern theories and views in the origin of the foliated rocks, and the minute zoning of the Upper Cretaceous rocks has been omitted.

In conclusion, while the author has introduced some important and novel features into his book, he would do well to prevail on his publishers to make a bonfire of certain venerable illustrations and to let the proof sheets of a new edition pass under the eyes of those who are familiar by actual field work with their several branches of the

science, in order to eliminate errors and supply omissions in a subject too wide to be adequately covered by a single individual, however varied his attainments.

THE LOWER OOLITES IN ENGLAND.

THE JURASSIC ROCKS OF BRITAIN. Vol. iv. Lower Oolitic Rocks of England (Yorkshire excepted), by H. B. Woodward, F.G.S. 8vo. Pp. xiv., 628, with plates i., ii., and woodcuts. Memoirs of the Geological Survey of the United Kingdom. London: Printed for Her Majesty's Stationery Office, 1894. Price 10s.

THIS is the companion volume to the one reviewed in NATURAL SCIENCE, vol. iv., p. 69, and it is by the same author. Whether the strictures then passed on the "get-up" of the volume have led to improvement, we will not presume to say; at any rate, the present volume shows a pleasing contrast to the earlier one, though it is still much below the standard befitting such a publication. The volume deals with the Lower Oolitic rocks of England, Yorkshire being excepted. Such a division of the geographical extent of the Lower Oolite may be convenient; but it seems to give undue prominence to Yorkshire, and to throw an unfair burden on the author of the present treatise. It is an immense, and, if we may say so, too ambitious a task for one man to cover so much ground. Such a proceeding may indeed possess the advantage that if the author be a strict palæontologist his readers know that, by whatever names he alludes to species, the same form receives the same name in different districts. This is, of course, not the case with different observers, and hence much of the difficulty of geological correlation results. Unfortunately, even this advantage is thrown away in the present instance. The weakest part of the whole work is the palæontology, not because of any want of ability on the part of those responsible for it, but because they cannot bring themselves to appreciate the needs of modern work. At the same time, the wisdom of expecting two or three men to grapple with all the present intricacies of palæontology, until separate monographs placing the subject in available form have been prepared, may well be doubted. In consequence of this policy the result is, as the Director-General says in a somewhat apologetic preface, "A general memoir which is intended to present a broad but detailed picture." We have no fault to find with this intention; but the detail should be accurate.

The scope of the volume is from the zone of *Ammonites jurensis* to the Cornbrash. The zone of *A. jurensis* was treated in the companion volume of the Lias. It is here included to form, with the *A. opalinus*-zone, a division, "Midford Sand," which is parted between the Lias and the Inferior Oolite—the term Midford Sand being qualified by the words "Yeovil" or "Cotteswold," according to locality, a concession which will be understood by those acquainted with the strata and with recent literature. Besides the zone of *A. opalinus*, only three other zones are admitted in the Inferior Oolite. The labour of further subdivision has been shirked, under the plea that other zones are only local—a curious plea, when some of those not admitted were traced across Europe thirty and forty years ago.

The whole of what is called "Great Oolite Series"—from Fuller's Earth to the Cornbrash—is divided into two zones, of which the Cornbrash takes one, that of *Ammonites macrocephalus*. Something seems to require explanation here. *A. macrocephalus* has usually been

regarded as indicating the zone above the Cornbrash. The author puts it (in the Appendix) as a fossil which ranges above the Cornbrash; and this admission seems to call in question the wisdom of the division adopted—a division not accepted by Continental authors. At the same time, it excites a doubt whether *A. macrocephalus* belongs to the Cornbrash, or whether the Cornbrash may not be only a lithological series of different palæontological age in different districts. We are inclined to ask, " What is Cornbrash ? ", well aware that every geological student preparing for his first examination would think himself able to instruct us.

In the "Great Oolite Series" we are introduced to a new term, "Fullonian," an impossible word from *fullonius*, which we hope will not be adopted. Only one Ammonite zone comprises the "Fullonian," and the greater part of the Bathonian. This is an extraordinary inversion of the value of terms, and it shows where work is required in this country. It also forcibly illustrates the lithological tendencies of the author. He can appreciate and recognise subdivisions where they are based on lithological characters, because these are so striking and catch the eye so readily that it would be impossible for anyone to overlook them. When, however, it comes to the more difficult task of looking beneath the surface, and understanding divisions made upon palæontological grounds, the author shows himself unable to perform it. Although the volume does not profess "to enter into minute particulars [which] belong to the domain of the specialist in some restricted field of palæontological research," yet it does enter into such particulars where lithology is concerned. Thus, as lithologist, the author is able to recognise eleven subdivisions of the Inferior Oolite; as palæontologist he can only see four. In our opinion, the volume marks the specialisation of the lithologist, and though we do not find fault with it on that account, for that is where its value lies as a scientific work, yet we take exception to the unequal treatment accorded to palæontology.

Perhaps the incorrectness of the palæontology is best exemplified in the woodcuts which illustrate the work. We confess to a difficulty in understanding why the woodcuts are given, because if the general geologist can identify his specimens therefrom he possesses skill denied to any specialist. Even if he do succeed he is no better off, for he will be wofully misled as to names. Why, when the original types were figured in British works, and when many of them are accessible in the British Museum, the woodcuts of what purport to be the same species should have been prepared from foreign figures, we are unable to understand. The result is certainly disastrous. Page 46 shows woodcuts of *Ammonites sowerbyi*, *A. murchisonæ*, and *A. humphriesianus*, all after d'Orbigny, whereas these fossils were named and figured by Sowerby. But d'Orbigny's *A. sowerbyi* disagrees in many respects with the type. His *A. murchisonæ* is not admitted to be that species at all, and his *A. humphriesianus* was named as a different species forty years ago by Oppel. Again, p. 48 shows a woodcut of *A. parkinsoni* after d'Orbigny, where a copy of Sowerby's type-figure might just as well have been given, and it would have saved the author the mistake of depicting as *A. parkinsoni* what has been separated by another name for some years. Worse, however, remains than this. Among the "Cornbrash fossils" shown in page 432, fig. 119 gives *Ammonites discus*, after d'Orbigny. This is another of Sowerby's species and the type is from the Cornbrash; but d'Orbigny figured as his fossil an Inferior Oolite shell. With unfortunate

temerity the author has taken upon himself to say that the Inferior Oolite species figured by d'Orbigny under a wrong identification is a Cornbrash fossil, and he has called it "*Ammonites discus*, Sowerby." A comparison of the suture lines shown by d'Orbigny and indicated by Sowerby ought to have put him on his guard. We fear, however, that such little details as suture lines do not accord with "individual views about species"—a phrase we find more than once in this work.

In the catalogue of fossils given in the appendix palæontological inaccuracy also reveals itself. It is not, however, by any means so conspicuous as was the case in the Liassic volume, and this, we take it, results from detailed work on Lower Oolitic species having been done by specialists in various recent publications. Thus the greatest and therefore most suspicious zonal ranges to be found in the tables are shown among the Lamellibranchiata, with which no British specialist has yet engaged himself, and we confidently expect that when one does these zonal ranges will be very much restricted. Outside the Lamellibranchiata very few species are given with wide zonal range—a marked contrast to the Liassic volume, while the majority are recorded from a single zone. A few exceptions occur, noticeably among the Brachiopoda. Thus *Rhynchonella concinna* and *R. obsoleta*, *Waldheimia anglica*, and *W. bullata* are given wide ranges. We venture to say that those who are making a study of Jurassic Brachiopoda—a by no means small band of naturalists in the West of England—will be unable to endorse these "finds" of the author. Nor will they wish to take from the Survey the credit of recording *Rhynchonella ringens* in the *Parkinsoni*-zone. We can understand some of the other zonal records by ascribing them to "individual views about species," but we confess to being entirely at a loss to know what *Rhynchonella* of the *Parkinsoni*-zone can have been mistaken for *R. ringens*, a species so remarkably distinctive in every way.

The stratigraphical work is also not free from error. Thus, in the section of Leckhampton Hill (p. 124), a considerable series of sandy beds, which should be between the Gryphite and lower *Trigonia* grits, are not shown at all. On page 110 something seems to have gone amiss with the zonal arrangement; for, while in the early part of the volume (p. 45) "the zone of *Humphriesianus*" is placed in the "upper division," here it is put to overlap a portion of the "lower division." Then the upper *Trigonia* grit is left out of account for zonal purposes altogether. The recognition of the zone of *A. humphriesianus* at all in the Cotteswolds may be regarded as an error founded on misconceptions of Wright and Witchell, and in making a quotation to support it (p. 111) about there being "no break of a marked character between the upper *Trigonia*-grit and the Gryphite grit," the author has done a little special pleading: the context insisted on there being a chronological break.

Although we have thus pointed out some of the pitfalls which have entrapped him, we hasten to place on record our conviction that, considering the immensity of the task upon which he has been engaged, the author has produced a work of great value to all those who may be studying the Lower Oolitic rocks. As a record of what has been done among these strata, accompanied by many new and important contributions from the author himself, as a guide to the localities and the rocks which they exhibit, and as a starting point for all new work, the volume is one of great interest. Our thanks are due to Mr. H. B. Woodward for the amount of information which he has rendered so readily accessible to the student.

Fossil Plants.

Catalogue of the Mesozoic Plants in the Department of Geology, British Museum (Natural History). The Wealden Flora. Part I.—Thallophyta–Pteridophyta. By A. C. Seward, M.A., F.G.S. Pp. xl., 179, with 17 woodcuts in the text. Plates i.–xi. London, 1894. Printed by order of the Trustees.

This, like some other British Museum Catalogues, is not a mere list but a detailed description of the specimens, with numerous critical remarks on their systematic position, and a discussion of the present state of our knowledge of individual groups. In an introduction of some five-and-twenty pages, the author summarises the results of previous work on the Wealden flora, and gives an account of records of fossil plants from other countries, but of presumably the same or similar age. Some recent collections of Mr. P. Rufford from the neighbourhood of Hastings have supplied much of the material for the present volume, but reference is also made to the collections of Mantell, Dawson, and Beckles. Three genera and twelve new species are instituted.

While commending the thoroughness of the work, and the care with which facts previously known are considered in the light of fresh knowledge, we would at the same time draw attention to one or two points affecting mainly the principle of the book or its arrangement. Thus, while it is of the utmost importance to quote synonymy, it is well also to avoid such a waste of space as is entailed by the chronological arrangement which has been adopted. For instance, under *Weichselia Manielli*, we have an account of the annual variation in the naming, from 1825 to 1890, occupying nearly two pages (pp. 114–16), in which one synonym, *Lonchopteris Mantelli*, occurs fourteen times. Surely it would have been better to lump all the references to this name in one paragraph. Similarly, on p. 27, under *Equisetites Burchardti*, this name occurs a dozen times. And why are we made to seek in the obscurity of the text for references to the plates?

The description of individual specimens is, we think, sometimes carried to an excess. For instance, under the species just mentioned, we find entries such as these:

V. 1070 and V. 1070a. A short piece of stem with three tubers, and another piece with four.

V. 1070b. Fragments of stems and several tubers. Two tubers shown in contact.

V. 2818 and V. 2819. Fragment of the same species.

As each item means a paragraph, no little space is thus occupied, whereas a bare quotation of the register numbers would have served the purpose.

A far more important point, and one affecting a great deal of recent palæontological work, is the foundation of species on material, the insufficiency of which the botanist shudders to contemplate. To give one instance: *Equisetites Yokoyamæ*, sp. nov., is founded on some tubers and fragments of stems, the tubers differing from a species previously described "in their smaller size and narrower elliptical form." It is well to note such differences, but to erect a new species without the support of other evidence, save locality, is to prejudice the undoubted value of the work as a whole. By the description of new species from often badly defined fragments of fronds, or other vegetative organs, specific names are reduced to marks of identification of individual specimens, and a comparison of facts in recent and fossil botany is rendered extremely laborious or practically impossible.

Meteorology in Maryland.

The Climatology and Physical Features of Maryland. First Biennial Report of the Maryland State Weather Service. Director, William Bullock Clark. 8vo. Pp. 140, 5 folding maps. Baltimore, 1893.

In our February number (vol. iv., p. 146) we reviewed an Outline of the Geology and Physical Features of Maryland, which, it will be remembered, contained much information as to the climate of the State. In the present Report the various meteorological data are given in much more detail, with careful analyses of the weather in different parts of the State during the years 1892 and 1893, and with tables showing the daily rainfall at 27 stations. The agricultural and practical value of this portion is greatly enhanced by a detailed account of the state of the various crops as dependent on the changing weather during the period in question. The general interest of the work, however, lies in the relations which it establishes between the leading features of the climate and the topography, and therefore also the geology, of the State. There is naturally a great difference between the western part of the State, or Appalachian region, and the lower lying Piedmont Plateau and Coastal Plain (see our previous review). Thus the average annual temperature of the extreme western region is 50° Fahr., that of the eastern border 58°, while even more pronounced variations are seen when the comparison is made by seasons. Similarly the average yearly rainfall of western Maryland is about 38·5 inches, while that of the remainder of the State is nearly 44 inches. A more detailed comparison brings out still more interesting facts. The lie of the isotherms, for instance, both annual and seasonal, is directly traceable to the configuration of the country. Variations of rainfall too can, as a rule, be explained by similar reasons. "Thus the broad Frederick Valley, lying as it does on the eastern flank of the Blue Ridge, receives an abundant rainfall from the moist south-easterly winds." At the same time there are many striking local variations in precipitation that have not yet received their explanation, and Professor Clark believes that they cannot be due to topographic differences.

Of the value of this work, which has only been in progress since May, 1891, we cannot speak too highly, and we are the more inclined to give good words since so much of the labour is voluntary. It is not merely to the Maryland farmers that these reports will prove of service: their educational value for schools, as showing in a comprehensible manner so many natural sequences of cause and effect, is not to be exaggerated; while we have no doubt that their usefulness to foreign meteorologists as records of fact will fully rival the services they will render to the State itself by attracting immigrants to a country so highly favoured of the clerk of the weather.

Congrès Géologique International. Compte Rendu de la 5me Session, Washington, 1891. 8vo. Pp. x., 530, maps and illustrations. Washington: Imprimerie du Gouvernement, 1893.

This ponderous volume of the proceedings of the fifth Geological Congress, held at Washington in 1891, reached England on the 23rd June. It contains reports of the various discussions on Pleistocene and other strata, the colouring of geological maps, the international bibliography of geology (which is in the press, and will be distributed by the author, M. de Margerie, as soon as ready), and a full report of

the excursions made by the Congress, dealing somewhat extensively with the geology of the western United States. The excursions, three in number, comprised the following: (*a*) The neighbourhood of Washington; (*b*) the Rocky Mountains; (*c*) Lake Superior, and the reports occupy some 330 pages. Maps, sections, and illustrations are given in quantity, as well as a sketch of the literature of each district, and the volume thus possesses the usual permanent value of these reports. It is a pity, however, that so long a time has lapsed before publication.

The Journal of Marine Zoology and Microscopy (May, 1894) continues to grow apace both in size and interest. This new number contains papers on "Sexual Colouring in the Cuckoo Wrasse," "The Significance of Dual Opercula in Serpulids," "Abnormal Muscular Bands in Salpa," as well as descriptions of the beautiful microscopical preparations sent out to the subscribers. The slides sent out this time contain the Phyllosome stage of *Scyllarus*, and four sections illustrative of the structure of the Anemones. That the Jersey Biological Station is progressing is only what we should expect, the situation being especially advantageous for the collection of marine objects. That it has many friends is evident by the ready response that has been made to a request for donations to the Library. We heartily wish Messrs. Sinel and Hornell the success they deserve.

MR. G. S. PERRIN sends us a reprint of a paper on Australian Timbers, read before the Royal Victorian Institute of Architects. The species of *Eucalyptus* supply the greater number of the useful woods, one of the commonest and best being the Blue Gum (*E. globulus*). The Hum or Macquarie Harbour Pine (*Dacrydium Franklinii*) is said to take precedence of all Tasmanian timber for lasting qualities, beauty, and easiness of working, while Western Australia has a large source of wealth in her immense *jawah* forests (*Eucalyptus marginata*). The author lays stress on the great importance of a rational conservation of forests, suggesting that when the reckless waste in America has caused a serious shortening of the timber export from that country, Australia will be looked to to supply the deficiency.

THE Jura of extra-European countries is receiving attention. The *Zeitschrift der Deutschen Geologischen Gesellschaft*, Bd. xlvi., Heft. 1, contains a paper by Dr. K. Futterer on the Jura of East Africa, with six plates chiefly devoted to the illustration of Ammonites. The fossils obtained from Mombassa, Tanga, etc., are considered to indicate beds of Callovian, Oxfordian, Kimeridgian and Tithonian age.

MR. J. T. CUNNINGHAM has two interesting papers in the last number of the journal of the Marine Biological Association. The first, on "The Life History of the Pilchard," traces the development of this fish from the egg until nine days old, when the observations were stopped by the death of all the specimens. The second paper deals with the ovaries of fishes, and refers to those of the plaice, dab, flounder, sole, turbot, and brill.

FROM Mr. David Nutt we have received Schröter's "Coloured Vade-mecum to the Alpine Flora," fourth edition, price 7s. This is a handy guide to alpine flowers, and will be of considerable service

to tourists in enabling them to identify their specimens. The serious student will not, of course, forget his Gremli, so indispensable to minuter study.

MM. J. B. Baillière et Fils, 19 Rue Hautefeuille, Paris, write to tell us that they have nearly ready for publication a Bibliography of French Flora, containing the titles of nearly one thousand books and pamphlets dealing with the subject. These are classified according to geographical regions. The publishers will send it free to anyone writing for it.

The *Nuovo Giornale Botanico Italiano* commences a new series with the present year. The original publication, which dates from 1869, was edited till 1872 by Beccari, but for the last two-and-twenty years has appeared under the direction of Professor T. Carnel. Henceforth it becomes the special organ of the Italian Botanical Society, and will contain those papers which are too large for publication in the Society's Bulletin. This is indicated by the addition to the title, "Nuova Serie," "Memorie della Società botanica italiana."

Under the title of "The Land of Viti," Mr. J. P. Thomson's paper on the Fiji Islands, read before the Adelaide Meeting of the Australian Association for the Advancement of Science (September, 1893), has been printed in the *Scottish Geographical Magazine* for March. Mr. Thomson deals with the various geographical divisions of the group, the coral reefs, geology, products, fauna, flora, natives, and climate. The paper is long and interesting.

We have received from Messrs. Williams and Norgate the issue of their foreign book circular for June, 1894, being no. 59 of their Scientific Series. It contains a classified list of recent foreign scientific publications, with the net cash price for which they can supply each book. Purchasers of scientific books will find it very convenient.

The July number of the *Scottish Geographical Magazine* contains articles and news of considerable interest and value. Dr. Otto Pettersson continues the account of recent Swedish hydrographical research in the Baltic and North Seas. In the last number of Natural Science, vol. v., pp. 5–7, we gave an account of the first part of Dr. Pettersson's review. This new part contains much detailed information, and is illustrated by many valuable charts of the degrees of salinity, the direction of currents, the contours of the sea-bottom, etc., of the areas investigated. Mr. E. Delmar Morgan writes of the mountain systems of Central Asia, and a useful coloured map is annexed. Mr. D. R. Urquhart continues his interesting notes upon the Bolivian *Altiplanicie.*

OBITUARY.

BRIAN HOUGHTON HODGSON.

Born 1800. Died May 23, 1894.

MORE than a third of a century has passed away and a new generation has arisen since the last contribution to Natural History from Brian Hodgson's pen was originally published, and it is more than half a century since he ceased to take an active part, as an official, in the work of the Indian Government. To appreciate the services that he rendered to Natural Science in the first half of the present century, it is necessary to recall the change, both in our knowledge of India and in our means of communication with it, that has taken place in the interval. One instance will suffice. At the time when Mr. Hodgson investigated the religions and languages, and described the mammals and birds of Nepal, so little was known of the Himalayas that their peaks, which exceed by many thousands of feet all other mountains on the earth, were supposed to be inferior in elevation to those of the Andes.

A few words will be sufficient for a sketch of Mr. Hodgson's career. He was the son of a banker, and was born at Macclesfield; he entered Haileybury in 1816, and landed in Calcutta to join the Indian Civil Service in 1818. In 1820 he was appointed secretary to the Resident at Katmandu, in Nepal; he became Resident in 1831, and occupied the post till 1843, when he was replaced by an even better known Indian official, afterwards Sir Henry Lawrence. Mr. Hodgson resigned the Indian service, and for a time returned to Europe, but in 1845 he took up his residence at Darjiling, a hill station that had only been occupied for about five years, and he remained there, partly engaged in studying the birds and mammals, but chiefly occupied with linguistic enquiries, until he finally left India in 1858. After his return to England he entirely relinquished the scientific pursuits on which his widespread reputation was founded.

There is no richer mammal and bird fauna in the world than that of the Eastern Himalayas, and if, when Mr. Hodgson arrived in Nepal, he entered on a zoological Eldorado, he made superb use of the opportunities he enjoyed. For many years the *Asiatic Researches* and the *Journal of the Asiatic Society* were filled with descriptions of new birds and new mammals from his fertile pen, and many of his communications appeared in the *Proceedings of the Zoological Society*

and in other periodicals. His papers, as a rule, were far from being mere descriptions of new forms; they contained numerous notes on anatomy, affinities, and habits. He published no large work, although for many years he proposed to bring out an account of the Nepalese bird and mammal fauna, with coloured illustrations. A large number of coloured drawings that had been prepared for this work by native artists under his direction were presented by Mr. Hodgson, together with the types described by him and the bulk of his collections, to the British Museum. In addition to these, he presented numerous specimens to the Museums of Calcutta, Paris, Leyden, Edinburgh, Dublin, etc. As a collector, indeed, he was at the time unrivalled. In the "List of the Specimens of Mammalia in the Collection of the British Museum," published in 1843, Mr. Hodgson's name is attached, in the "Index of Donations," to a larger number of references than any other donor's, and at this time only his first contribution to the national collection had been received. Subsequently, two separate catalogues of his presented collections were published, one in 1846, and the other in 1863.

A better idea of Mr. Hodgson's energy than any that can be derived from lists of specimens or even from a perusal of his papers is afforded by the drawings presented by him to the British Museum, or, still better, by the original copies that have found an appropriate resting-place in the Library of the Zoological Society of London. These drawings represent many hundreds of mammals and birds, and fill several large folio volumes, the same species being sometimes drawn three or four times. Each sheet, besides the figure of the whole animal, generally contains drawings of details of the external and internal structure, and the paper is crowded with manuscript notes on the localities, habits of life, breeding, nidification, and measurements. No better example of the care with which Mr. Hodgson collected facts could be cited than his paper "On Various Genera of Ruminants," published in 1847 (*J.A.S.B.*, xvi., p. 685). In some respects he was in advance of the science of the day. He was fully alive to the importance of geographical distribution, and was the first to attempt a demarcation of the zones of life, resulting from differences of elevation, in the Himalayas. Unfortunately, his collections, when incorporated with those of the British and other Museums, were indiscriminately labelled Nepal, whether they came from that country, from Sikhim, or from the plains of India. Serious confusion has resulted from this mistake and from others of a similar kind.

Mr. Hodgson's researches in Natural History were by no means his only claim to recognition. He was even better known for his enquiries into the languages, literature, and religion of Nepal than for his study of the fauna. He has been regarded as almost the discoverer of Tibetan Buddhism, and he certainly was the first to call attention to its literature. During the years that he lived at Darjiling, in the dwelling so admirably described in Hooker's

Himalayan journals—one of the most beautiful sites to be found, even in that magnificent range—by far the greater part of his time was devoted to the collection of the unwritten languages spoken by the scattered and rapidly disappearing aboriginal tribes of India, from the Himalayas to Ceylon. Nor were his diplomatic services small; among them it may be remembered that he was the first to advocate the enrolment of Ghurka sepoys, and to appreciate the merits of one of the most gallant races of Asia.

Owing to the interest excited in Paris, then a great centre of Oriental studies, by his researches in ethnology and literature, Mr. Hodgson became, as long since as 1838, a corresponding member of the Institute, and received the Cross of the Legion of Honour and a special medal struck by the French Asiatic Society. In England the recognition of his services to science was tardy and incomplete. He became a fellow of the Royal Society in 1877, and when he was in his ninetieth year the University of Oxford conferred honour on itself by investing him with the degree of D.C.L. It is to be regretted that no place has been found for him in the long roll of honours conferred by her Majesty for services in India.

CARL JOSEPH HYRTL.

Born 1811. Died July 17, 1894.

AMONG others who have passed away recently is Carl Joseph Hyrtl, the anatomist, of Vienna. Dr. Hyrtl was almost as famous a teacher as an investigator. He aroused an extraordinary enthusiasm in his students, his classes being attended not merely by those qualifying for medical work, but by those already advanced in years and eminent in their profession. He was also celebrated for his anatomical preparations, which, scattered in all directions, bear eloquent testimony to his energy and powers. Dr. Hyrtl had a remarkable knowledge of languages, speaking Greek and Latin as well as several modern languages. He died at Perchtoldsdorf, near Vienna, where he had retired when the affliction of blindness closed his earthly labours. He is best known to the public by his "Anatomy of Man," and "Topographical Anatomy." His other works dealt with the anatomy of mammals, reptilia, and fishes.

Ferdinand Heine, the founder of the Ornithological Museum which bears his name, died on 28 March at Halberstadt; Lucien François Lethierry, the well-known student of Hemiptera, died at Lille on April 4, at the age of 64; Edward Norton, the entomologist and specialist on Hymenoptera, on April 8, at Farmington, Conn., U.S.A., at the age of 70 years; Dr. F. Quiroga y Rodriquez, Professor of Crystallography at the University of Madrid, on June 3.

Dr. Quiroga also contributed to our knowledge of the geology of the interior of Africa.

CARL THEODOR LIEBE, the geologist and ornithologist of Thuringia, passed away at Gera on June 5; and EDOUARD LEFÈVRE, the Secretary of the Société Entomologique de France, and a well-known Coleopterist, died at Paris on June 18, aged 55 years.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Dr. J. Walther has been appointed Professor of Geology and Palæontology at the University of Jena; Dr. A. Tenne, Professor of Mineralogy at the University of Berlin; Dr. F. Altmann, Professor of Botany at the University of Freiburg i.B.; Dr. A. König, Professor of Zoology at the University of Bonn; and Dr. R. Scharizer, Professor of Mineralogy at the University of Czernowitz.

Mr. P. Chalmers Mitchell, M.A., F.Z.S., lecturer in Biology at the Charing Cross Hospital Medical School, has been appointed lecturer in Biology at the London Hospital Medical College.

Mr. R. Lydekker, F.R.S., is again in the Argentine Republic. He is engaged on the further study of the palæontological riches of the La Plata Museum.

Dr. C. J. Forsyth Major, accompanied by Mr. A. Robert, starts for Madagascar at the end of this month under the auspices of the Royal Society. Dr. Major will endeavour to complete our knowledge of the extinct animals that have inhabited the island, and his special qualifications, both as a zoologist and an explorer, allow us to hope for considerable and valuable results. Dr. Major is a Scotsman, and was born in Glasgow; as now he belongs to every country, we hope he will have little trouble with any of the powers that be.

Mr. F. W. W. Howell proposes to make a third exploration of Iceland. The district to receive attention is the Northern Glacial district of the Vatna Jökull. The funds necessary Mr. Howell proposes to raise by promising a lecture on his return to any society or institution for a fee of five guineas.

Our statement that Dr. R. Semon had left the Anatomical Institute at Jena was gleaned from an erroneous source; although Drs. Braus and Drüner have become assistants at that establishment, it will not lose the valued services of Professor Semon.

The following eminent botanists have promised to attend the meeting of the British Association:—Dr. Strassburger, Dr. Zacharias, Dr. Errera, Dr. Pfeffer, and Mr. Douglas H. Campbell.

Mr. George Holt, of Liverpool, has given £5,000 to the University College of that town, which is to be invested to maintain the pathological and bacteriological laboratories. Professor Boyce, of University College, has been elected by the council to fill the newly-created post.

The generosity of Ludwig Mond, F.R.S., has placed at the disposal of the Royal Institution the freehold of No. 20 Albemarle Street, to be held for the purpose of a laboratory, to be named "The Davy-Faraday Research Laboratory of the Royal Institution." Mr. Mond further offers to make all necessary structural alterations and to equip the laboratory with apparatus, appliances, etc.

A GOVERNMENT Museum for Natural History and Ethnology has been established at Para, in North Brazil, and Dr. Emil A. Goldi has been appointed its first Director.

THE Library and Reading-Room of the Linnean Society will be closed during the month of August for cleaning.

THE Zoological Society of Dublin have received, on deposit from Mr. Cross, of Liverpool, an Orang Utang. The attendance at the Gardens during the month of May reached 11,000.

THE *Deutsche Fischerei-Verein* offers a prize of 1,000 marks for a paper embodying the best researches on the pathological and anatomical influence exerted on fish by the presence of the following substances in the waters that they inhabit:—(1) Free acids, (2) free bases, especially lime, ammonia, and soda, including the soluble carbonates of potash and soda, (3) free bleaching gases (chlorine and sulphurous acid). The determination of pathological characters accompanying the death of fish from asphyxia is also desired. As appropriate subjects the Salmonidæ and Cyprinidæ are suggested. Papers, which may be in German, French, or English, must be sent in by 1st November, 1896, to Professor Dr. Weigelt, Zimmerstrasse 90/91, Berlin, S.W., and from him further details may be obtained.

AT its meeting in Munich, the German Zoological Society drew up the final programme for the great systematic work to which we have alluded (NATURAL SCIENCE, Nov., 1893, p. 383). The work, which will be published by G. Fischer, of Jena, is to be entitled "Das Tierreich. Eine Zusammenstellung und Kennzeichnung der rezenten Tierformen." Though excluding animals properly described as fossil, the work will take cognisance of such forms as have become extinct during historic times. Each species will be accompanied by a diagnosis, as brief and clear as possible. We are glad to find it recognised that such a work can only attempt to be a synopsis of actually published knowledge, and that no corrections or new descriptions can be admitted. The direction of this gigantic task is entrusted to Professor F. E. Schulze, of Berlin, who will be assisted by a committee of seven and a number of sub-editors.

THE Geological Society of London has raised its Composition Fee. We give the new bye-law which was passed at a special general meeting held on June 20, 1894. "A fellow may at any time compound for future annual contributions, that of the current year inclusive, by payment of thirty-five pounds, or if elected before the 1st November, 1894, by a payment of thirty-one pounds ten shillings, or if elected before the 1st November, 1877, by a payment of twenty-one pounds. If he has already paid the contribution for the current year, or any part of it, such payment shall be reckoned as forming a portion of the composition."

THE Geologists' Association of London holds its long excursion this year in Shropshire. The programme, which is elaborate and lengthy, speaks well for the energy of the leaders and the excursion secretary, Mr. Thomas Leighton. Members will have an opportunity of inspecting in the field the whole sequence of rocks below the Bunter Sandstone, and that under the leadership of such men as Lapworth, Watts, Blake, Callaway, and La Touche. The excursion lasts from July 28 till August 4. The headquarters will be Shrewsbury.

WE learn from *Science Gossip* that a Geologists' Association has been formed at Bristol, which holds meetings and makes excursions on Sundays. The honorary secretary is Mr. Frederick Ellis, 22 Senier Street, Bristol.

The Hundred-and-tenth Meeting of the Yorkshire Naturalists' Union was held at Knaresborough, on Saturday, July 14, 1894, for the investigation of the Nidd Valley from Nidd Viaduct to Goldsborough Mill.

The meeting of the French Association for the Advancement of Science will take place at Caen, from the 9th to the 15th August; the Geological Society of France will hold their extraordinary meeting at Lyons from the 19th to the 26th August; and the Botanical Societies of France and Switzerland at Geneva and the Valais from the 5th to the 14th August.

The meeting of the Museums' Association at Dublin called out some interesting papers. Mr. H. B. White of the Dublin Museum described certain of the fittings and appliances there used. Mr. W. E. Hoyle, of the Manchester Museum, described some beautifully arranged cases, illustrative of the structure of the Foraminifera and the classification of the Pelecypoda; the latter explain the two different systems of arrangement according to shell-teeth and gill-structure. Messrs. Hoyle and Bolton explained an elaborate system of cataloguing fossils on a modification of Dewey's decimal method. Mr. G. H. Carpenter showed some attractive cases that he has arranged in the Dublin Museum to illustrate the chief features in the struggle for life, evolution, and geographical distribution of animals. Mr. H. O. Forbes, of Liverpool, advocated the centralisation of type-specimens in metropolitan museums, a suggestion that gave rise to a lively discussion. Mr. F. A. Bather, of the British Museum, had a great quantity of useful information, collected from museums in different distant parts of the world, which he endeavoured to impart to his colleagues. Professor T. Johnson, of Dublin, upheld the value of a Botanical Museum, especially in its relation to agriculture.

The Missouri Geological Survey has been in trouble, and we regret to learn that Mr. Arthur Winslow has been forced by circumstances to retire from its directorship. In his successor, however, Dr. C. R. Keyes, who has lately done much good work on the Iowa Survey, the State secures a skilled observer already well acquainted with the geology of Missouri.

Those interested in the United States Geological Survey will find an appreciative notice of Major Powell's administration in the May-June number of the *Journal of Geology*. The writer, Professor T. C. Chamberlin, dwells chiefly on the value of a topographic survey inspired by a feeling for the genesis of physiographic features.

The "Societá Geologica Italiana," which was started in 1881 at Rome, now numbers 223 members. In the last number of its *Bolletino* (anno xii., fas. 4) will be found a series of papers dealing with the geology of Piedmont, more especially with the fossil Foraminifera, Coniferæ, etc., and a geological bibliography of some 1,100 entries.

The "Society for the Study of the Amur Regions" has now become the nucleus of a new branch of the Russian Geographical Society. It will have its headquarters at Khaborovsk, and be known as "The Amur Branch." A yearly subsidy of 2,000 roubles will be given by the Government.

The "Recent Territorial Arrangements in Africa" is the title of a short paper by Mr. Ravenstein in the *Geographical Journal* for July. In it are given two maps, one of the Congo State, and another of the Somali-land area, to illustrate the present arrangements come to by the eagles over the carcass of Africa.

In his anniversary address delivered to the Royal Geographical Society, Mr. Clements R. Markham referred to the new catalogue of the library of the Society, which will be in the hands of fellows some time this year. This catalogue has now been arranged in one alphabetical order, and there will be two appendices, the first containing an alphabetical list of all the collections of voyages and travels, with an analytical table of contents to each volume; the second appendix containing anonymous and periodical literature arranged in geographical order. These catalogues have been prepared by Dr. Mill, Mr. Vincent Hawkins, and Dr. Murie.

A still more useful catalogue is being prepared for the same Society by Dr. Mill and Dr. Murie. That is a subject catalogue which is to be at once exhaustive, systematic, and exclusively geographical. Such a book as this, if properly carried out, will be an invaluable contribution to geography, serving as a guide to workers in all parts of the world. But, of course, it involves great labour. The number of titles to be indexed Dr. Murie calculates at 110,000, and this will print into something like 5,000 pages octavo. It is hoped that this book will be ready in about two years, and one might perhaps indulge the hope that Dr. Murie will keep an especial eye on bibliographies.

Dr. R. Lloyd Praeger has written in the July number of the *Irish Naturalist* a sketch of the history of the Belfast Naturalists' Field Club. This is but the first of what must be a most interesting series of histories of the Irish Natural History Societies, and we shall await the succeeding papers with interest. The Belfast Naturalists' Field Club was the outcome of some courses of lectures on Natural Science by Mr., now Professor, Ralph Tate, and was started in 1863. Since then it has steadily increased in prosperity and usefulness, and has published many interesting and valuable papers in its well-known but somewhat complicated "Proceedings" and "Appendices." The membership is now just about 500.

Part I. of the fourth volume of the Actes Soc. Sci. Chili has just reached us. Mr. Boulenger has a paper on the fresh-water perches of Chili, and Dr. L. Vergara Flores one on the cranium of an aboriginal Bolivian. The Society numbers some 250 members, and publishes in its "Actes" many valuable observations relating to Chili. The library seems to be growing rapidly under the care of Dr. Briones. Parts 4 and 5 of volume iii., and parts 4 and 5 of volume ii. of the "Actes" will appear shortly, and then these volumes will be complete.

In a paper read before the Royal Irish Academy, Mr. R. J. Ussher shows that the Golden Eagle still breeds in Western Mayo, Donegal, Galway, and Kerry. The White-Tailed Eagle has been observed in Mayo and Kerry. The Pelegrine Falcon breeds fairly commonly throughout Ireland, but the common Buzzard is rare. Tree Sparrows also breed in co. Dublin, and are increasing in numbers. The Raven is everywhere rare, and the Bittern is now never to be seen.

Dr. C. E. Stirling, F.R.S., has contributed to *Nature* (June 21 and 28) an account of his remarkable discovery of the remains of *Diprotodon* and other extinct vertebrata at Lake Callabonna, South Australia.

CORRESPONDENCE.

Sewage or Filth-Fed "Fish."

Bordering on our coast, the sea being contaminated by ever-increasing sewage-outfalls, efflusive refuse and dirty rivers—too often only open drains artificially fouled from source to mouth—may sooner or later cause the *extinction* of the British trout and British migratory salmon by stopping their sojourn and spawning in such sewage-spoilt streams.

It was estimated in 1882 that local sewage and refuse had already so polluted and poisoned the rivers of England—reckoned roughly at about 60,000 square miles—that then (1882) upwards of one-sixth of these waters were incapable of supporting fish-life.

On the other hand, like some sea birds, many salt- and fresh-water fish frequent and are fond of a moderate quantity of, sewage, whose warmth, products, and contents also favour local aquatic vegetation.

Sewage-fed oysters, besides other aquatic and amphibian animals usually eaten either absolutely raw or else insufficiently cleaned and cooked, cause avoidable parasitic diseases, fatal fevers and poisoning, which details are discussed in my 1893 treatise on "Foul Fish and Filth Fevers," that the United States Fish Commission will shortly publish, and my article on "Sewage-Fed Fish" in *Public Health* of June, 1894.

It is imperative that immediately a Royal Commission, a Select Committee, or at least a Parliamentary Return should be granted to inquire into and report upon remedies to diminish the dangers to health, and the damage to property, food, and sport caused by the augmenting sewage pollution of our inland and coastal waters.

J. Lawrence-Hamilton, M.R.C.S.

Sun or Moon?

Is not *Orthagoriscus mola* usually considered a sun-fish (*vide* Günther "Study of Fishes," p. 690) not a "moon-fish," as stated by your reviewer on p. 71?

11 Wellington Road, Brighton. Edward Crane.

July 7th, 1894.

[Sun-fish or Moon-fish.—The paper we referred to was "Note sur un *Poisson-Lune* (*Orthagoriscus mola, L.*), de grandes dimensions, capturé sur les côtes du Portugal." What *we* know as the moon-fish is *Ephippius gigas* of the Antilles, but there is no reason for supposing that *Orthagorisci* are called sun-fish in all parts of the world.—Editor.]

A Three-Toed Passerine.

Sir,—I am much indebted to you for pointing out in your Editorial Note on my paper (Natural Science, vol. v., p. 10) the interesting fact of the persistence of the *flexor longus hallucis* in three-toed birds. The note contains, however, a slight mistake, viz., the statement that there is no three-toed passerine bird. *Cholornis*, a genus believed to be allied to *Suthora*, has but three toes. The suppressed toe, however, is unfortunately not the hallux, but the fourth toe. For a three-toed passerine of the kind you implied, I fear we must wait till an abnormality occurs.

Frank Finn.

Two Corrections.

We are indebted to Miss Agnes Crane and to another correspondent for pointing out that the paper by Dr. Meyer referred to in our last issue (p. 14) is not a new publication, but the first part of vol. iv. of the "Abhandlunge und Berichte des K. Zoologischen und Anthropologisch-Ethnographischen Museums zu Dresden." We are indebted to Miss Agnes Crane also for reminding us that the Senior Editor of the "Country Month by Month" is not the "Son of the Marshes," but Mrs. Owen, who has herself edited the writings of the "Son of the Marshes."

Editor.

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, 5 John Street, Bedford Row, London, W.C.

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NATURAL SCIENCE.

SPECIAL ILLUSTRATED SUPPLEMENT TO NO. 30, AUGUST, 1894.

Taxidermy as a Fine Art.

If any doubt the value of that connection between science and art to which we have referred on p. 90, then a glance at the fascinating Report by Dr. Shufeldt, which we reviewed in our July number (pp. 58–60), would convince them of their error, at least so far as Taxidermy is concerned. Dr. Shufeldt's plea is throughout for a more artistic rendering of the stuffed and modelled animals and groups in our museums; and when we inquire how this desirable result is to be attained, we find that it is solely by holding the mirror up to Nature herself. That this has not always been done is proved, if proof be needed, by several of the illustrations to the Report; while that it can be done by those with a wide knowledge of nature, artistic feeling, and command of technique, is sufficiently evidenced by many other of the beautiful plates in the book. Through the kindness of Dr. Brown Goode, Assistant Secretary of the Smithsonian Institution, we are now enabled to present our readers with a selection from these plates, to many of which we alluded in our review.

The specimen of *Octopus vulgaris* (Pl. I.) is a gelatine cast taken from a plaster mould, and then faithfully coloured according to nature. The mould was made, as is usual in these cases, not from the animal itself, but from a carefully prepared model, which in this instance was based on a figure by Verany. "It is hard," says Dr. Shufeldt, "to realise what a perfect representation one of these finished gelatine casts gives of the living animal; and, the cast being perfectly pliable, it still further enhances the resemblance to the original."

The Skate, *Raia erinacea* (Pl. II.), is a plaster cast taken from the animal, and coloured, the eyes and minor appendages being added after the cast is made. Some such process as this is the only one by which the large cartilaginous fishes can be reproduced with any marked fidelity to nature and fit for a first-class museum. "Rays,"

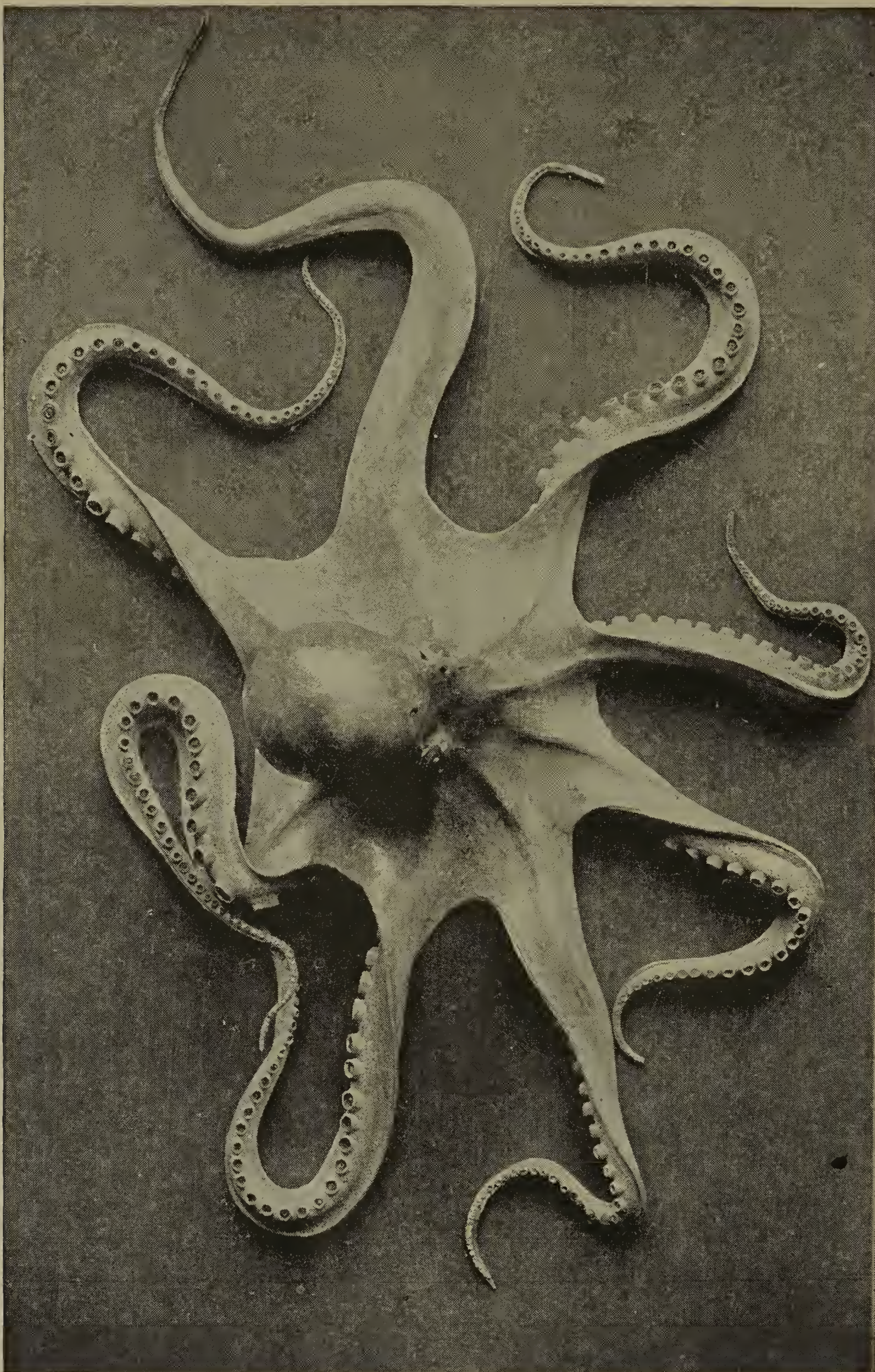
says Mr. Hornaday, "are the meanest of all subjects that vex the soul of a taxidermist. Such abominable animated pancakes, with razor edges that taper out to infinite nothingness, were never made to be mounted by any process known to mortal man. To mount the skin of a vile ray, and make it really perfect and lifelike, is to invite infinite shrinkage, rips, tears, warps, defeat, and humiliation at the hands of your envious rivals. . . . The best way to mount a ray is to make a nice plaster cast of it, paint it, and then bury the accursed ray in a compost heap."

The Rattlesnake, *Crotalus adamanteus* (Pl. III.), is an example of the beautiful plaster casts of reptiles, made by Mr. Joseph Palmer, which are a striking feature at the National Museum at Washington. "With tail elevated, and the reptile thrown into natural coils, partly within the recess of the spreading roots of a large tree, we have," says Dr. Shufeldt, "an accomplishment in plaster, the equal of which for that particular snake I do not believe to be extant."

Lizards, owing to the delicacy of their details, are far more difficult things to cast. "Nevertheless the plaster casts of some of the larger lizards leave us nothing to be desired in that art. A truly magnificent thing is seen in the plaster cast of *Tupinambis*. It would seem to be perfect in every particular." (Pl. IV.)

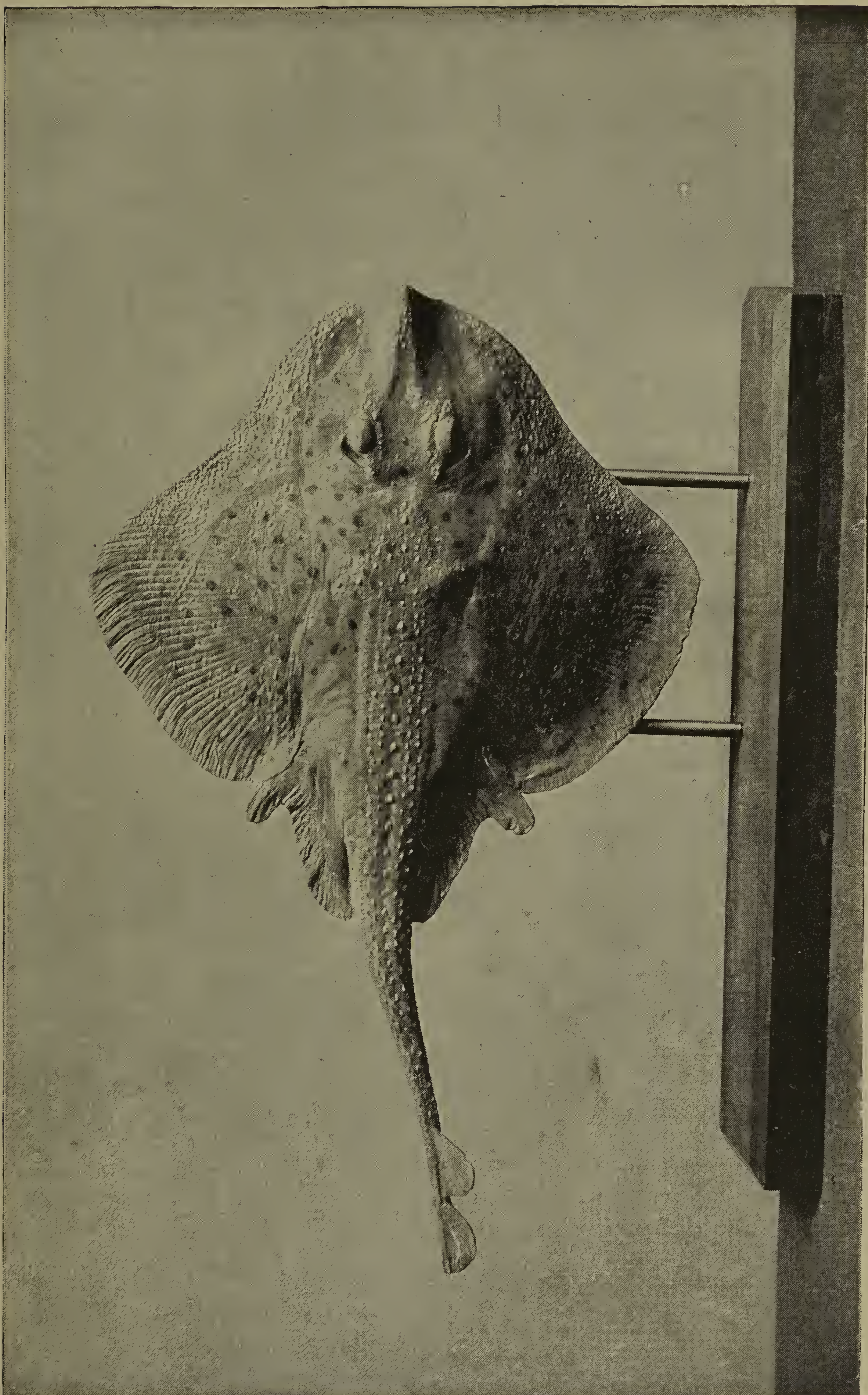
The figures of the Great Auk both represent the same specimen, which is owned by the Museum at Washington (Pl. V.) We again quote Dr. Shufeldt. "A figure of this as first mounted by some ancient bungler is shown. No living auk in good health ever stood in that position; but, thanks to what art can sometimes accomplish in these days, this outraged bird was not destined for all eternity to stand as a drum-major at dress parade. It was determined to have it remounted; an operation, owing to the age of the specimen and the lack of knowledge as to what condition the skin might be in, that required a full measure of judgment. The work of remodelling was accomplished by Mr. Wood." The result is that the extinct fowl "presents a far more respectable appearance, and is certainly posed in a far more natural attitude, though judging from Audubon's plate of it, I believe it still to be not a posture this auk was wont to assume. Still it was most assuredly the very best that could have been done under the circumstances."

The head of a Tiger (Pl. VI.) illustrates how well the great difficulties encountered in representing the mask of a living animal can be overcome. On this hear again the words of Dr. Shufeldt: "One who has not seen the feat performed in one of our larger museums can have but little conception of the skill required in handling the facial expression and all the structures that enter into the mouth parts. The skinning of a tiger's tongue and preserving it so as to make that organ resemble the original as it appeared in the living subject; the cleaning of the teeth; the blending of the black part of the lips with the delicate pink gums inside; to make the animal grin and not smile,



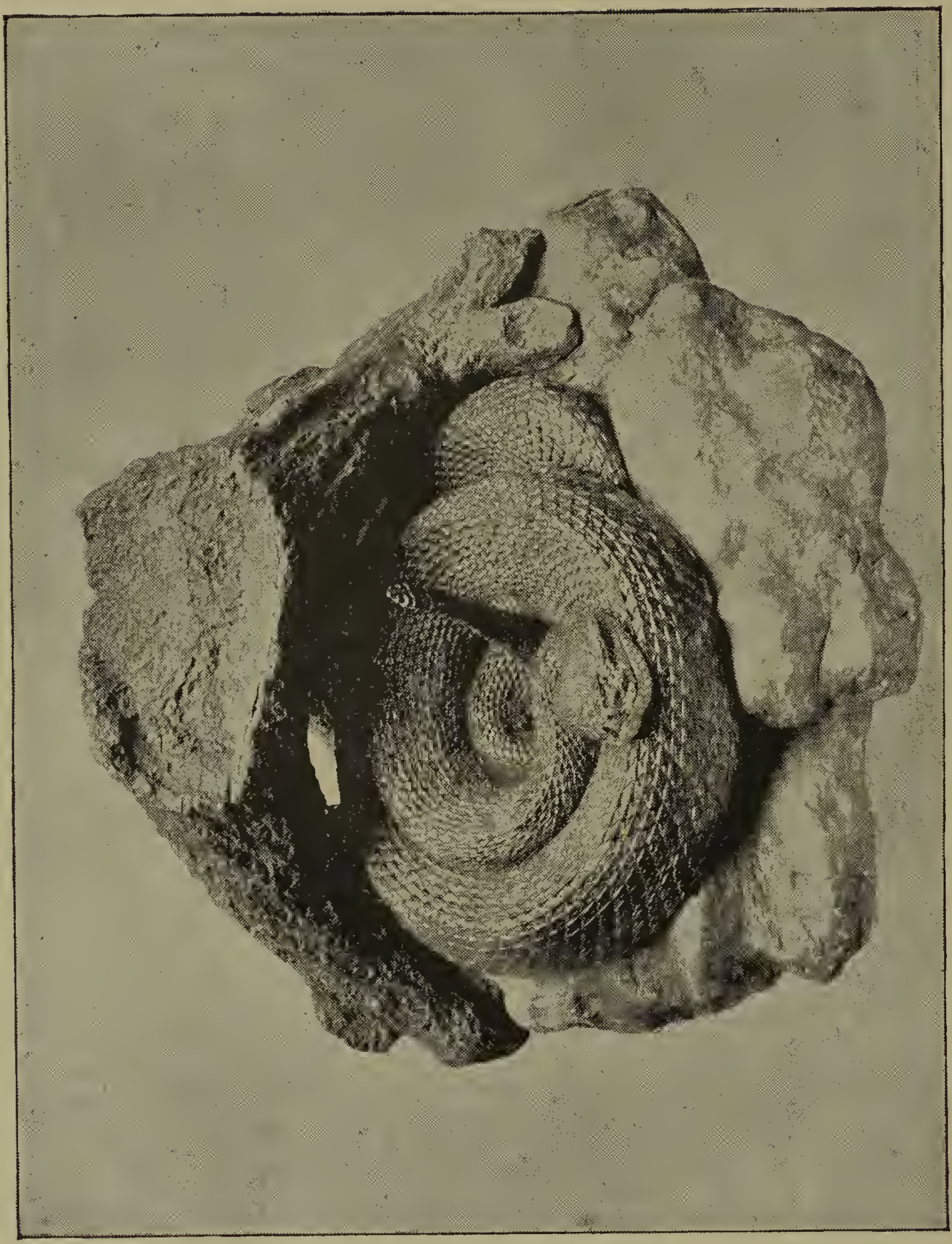
Octopus (*Octopus vulgaris*).

From a gelatine cast; reduced. U.S. National Museum.



SKATE (*Raia erinacea*).

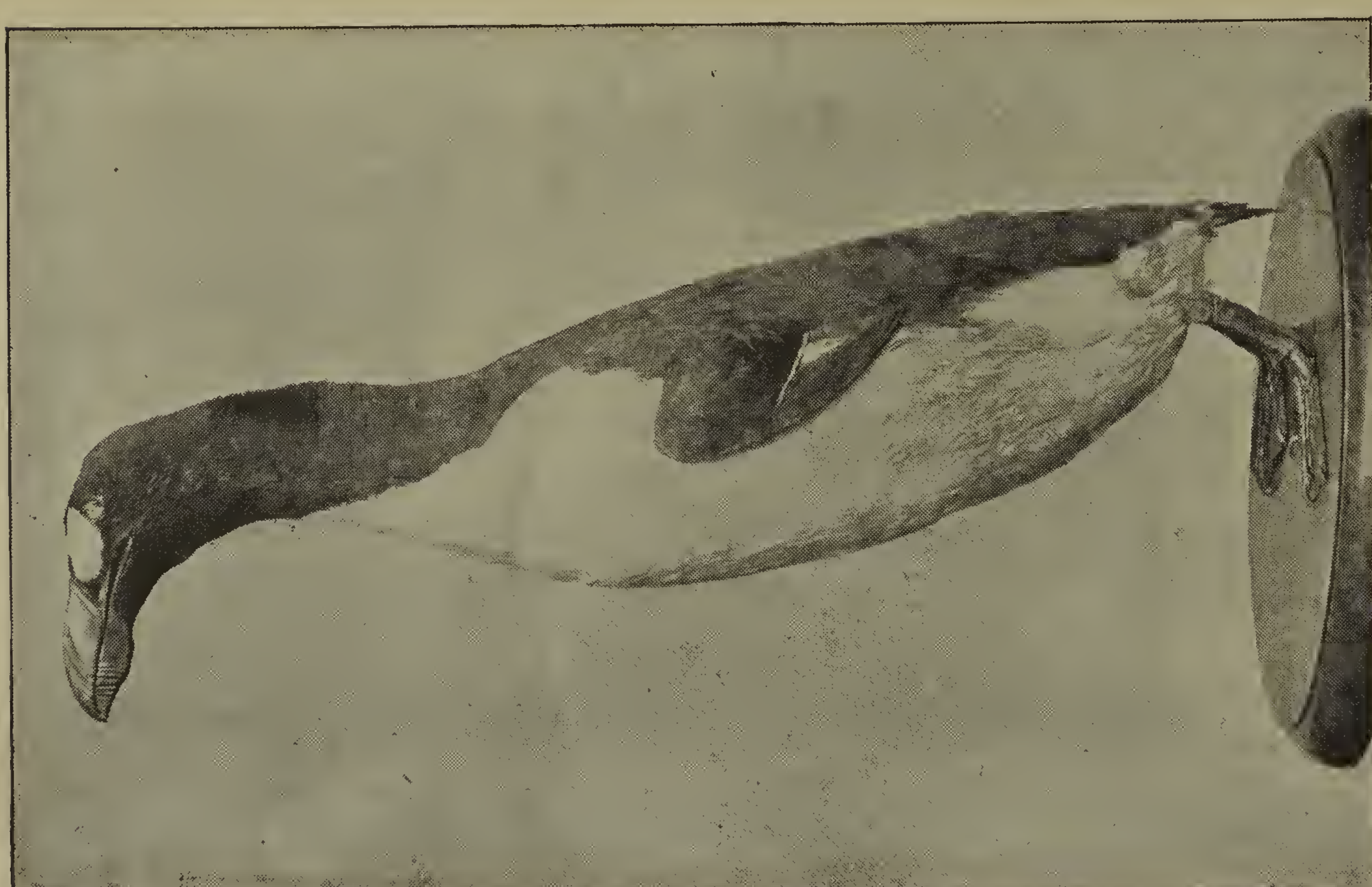
From a plaster cast; greatly reduced. U.S. National Museum.



RATTLESNAKE (*Crotalus adamanteus*).
From a plaster cast; greatly reduced. U.S. National Museum.

TUPINAMBIS TEGUIXIN.

From a plaster cast; reduced. U.S. National Museum.



As first preserved.

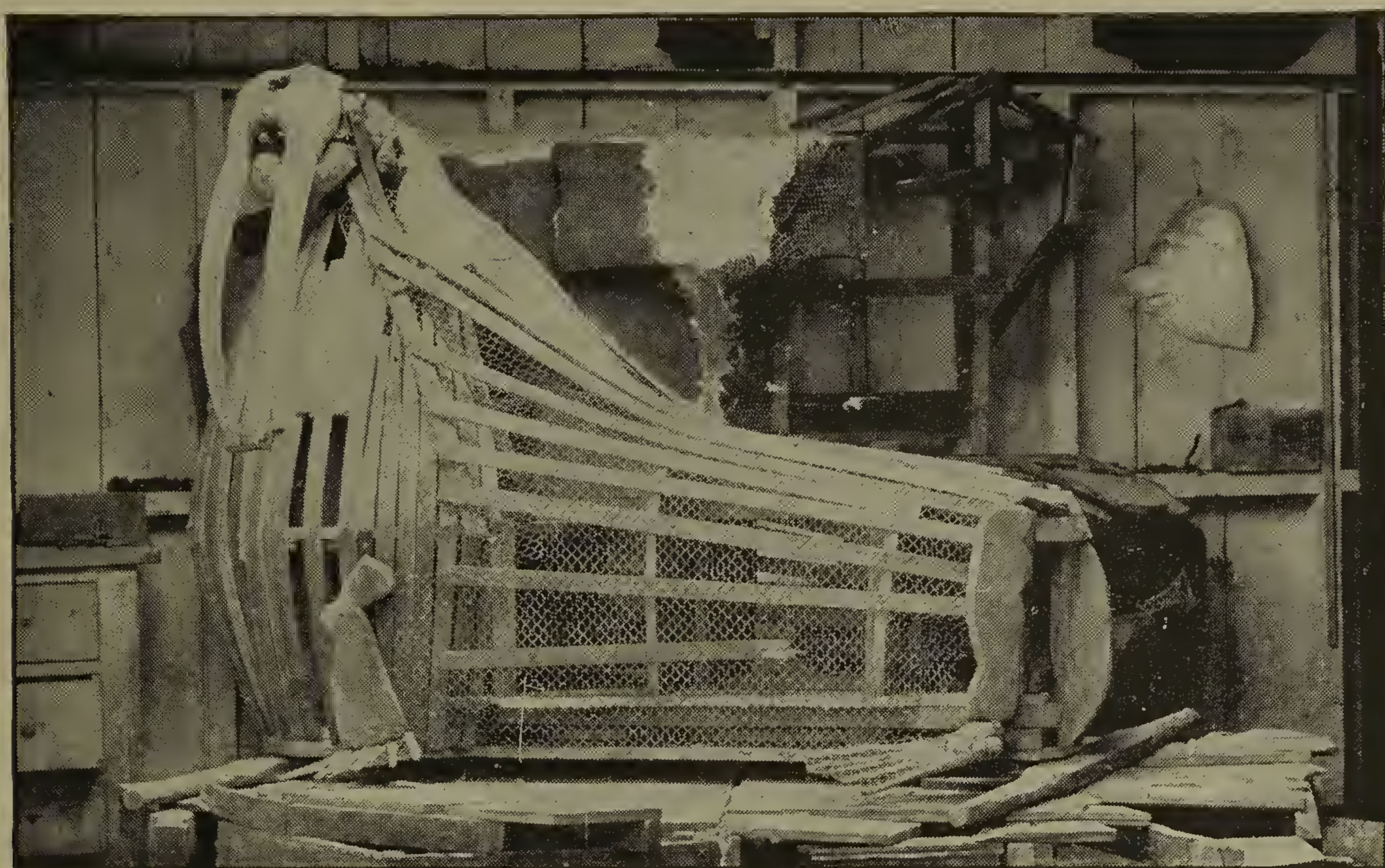
As remodelled by Mr. Nelson R. Wood.

THE GREAT AUK (*Plautus impennis*).
U S. National Museum.

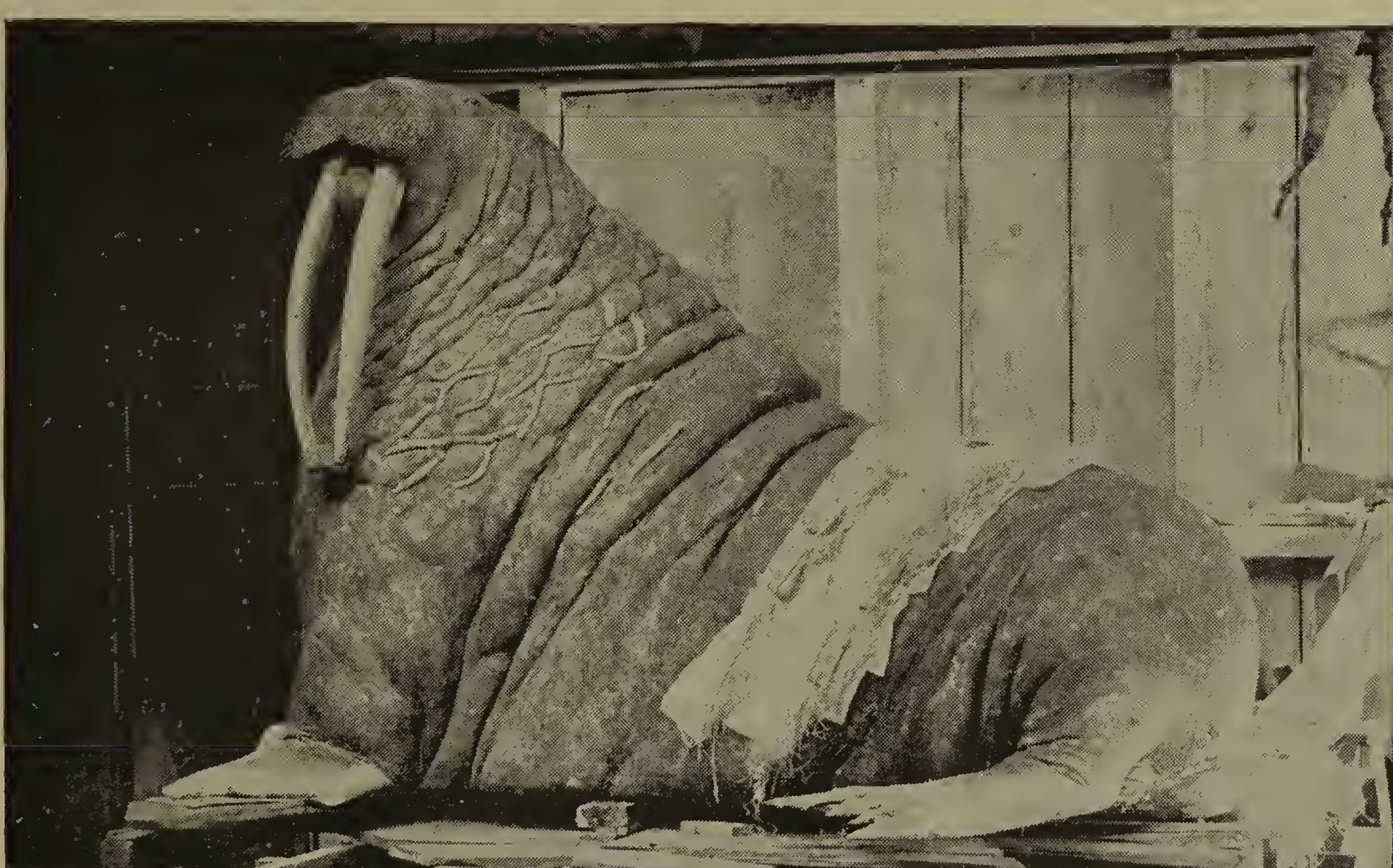
HEAD OF TIGER (*Felis tigris*).
U.S. National Museum.

HEAD OF ZEBRA (*Equus burchelli*).

U.S. National Museum.



MANIKIN FOR A WALRUS. (Partly completed.)



MALE WALRUS (*Odobænus obesus*). (Nearly completed.)
U.S. National Museum.

and to lend to the eyes the flash of anger, are all accomplishments that demand of the artist his best judgment, knowledge, skill, and, what is more, his infinite patience." Hear also that eminent artist in this line, Mr. James Hornaday. "The large Felidæ are the finest subjects for the taxidermist that the whole animal kingdom can produce. They offer the finest opportunities for the development of muscular anatomy, and the expression of the various higher passions."

A specimen in which difficulties of a similar nature have been admirably coped with is the Burchell's Zebra, of which the head is shown from the side in Plate VII. This is thus described by Dr. Shufeldt.—"The animal has been given an attitude indicative of moderate movement, with the evident idea in its mind of making an attack or standing at bay, in which he will use his teeth to bite—a habit so familiar to us in some cases of vicious horses. The short mane is semi-erect, the ears are thrown back, the eye looks the owner's intent, while the quivering and nearly rigid lips drawn apart show the glistening upper 'nippers' and the crowns of the lower ones; the nostrils are somewhat closed by the elevation of the superior lip; finally the entire rendering of the whole animal is most perfect in all particulars."

The two figures of the Walrus on Plate VIII. are of exceptional interest. This animal is now exceedingly rare and on the very verge of extinction. Familiar though it has been to us from our earliest childhood, yet we have learnt of late that the pictures of it in works on zoology and natural history, "even in so good and generally correct a work as Brehm, are glaringly false." Only in the last few years have truthful drawings, made from actual observation by Mr. H. W. Elliott, been available for the taxidermist to take as his models. According to Dr. Shufeldt, the specimen of this colossal mammal at the National Museum in Washington "was mounted in the light of all the improvements and skill modern taxidermy could bring to bear upon the undertaking, and the success was complete. It constituted, when finished, one of the grandest subjects the Smithsonian sent on to the Government exhibit at the Columbian Exposition." All opinions, however, are not so favourable; for Dr. W. H. Dall, as already quoted in NATURAL SCIENCE (vol. iii., p. 337), said,—"A fine walrus might have been more lifelike if the taxidermist had had a better guide than Elliott's caricatures of this unfortunate animal, which, in addition to extinction, seems to be doomed to posthumous misrepresentation." The figures, whatever may be the accuracy of the specimen, serve to illustrate the mode of stuffing by means of a manikin, which, especially in the case of hairless animals, is often covered with clay, which can be worked into shape after the skin has been put over it.

In selecting the plates for this article, we have paid but small attention to the many beautiful illustrations of birds. In respect to

bird-groups our home museums do not require much teaching, though even they have yet to learn that a bird can be mounted in the most natural manner on an ordinary museum-perch or stand. It is in preparing the other classes of Vertebrata and the Invertebrata that American taxidermists take the lead, and it is their excellence in this direction that we have endeavoured to set forth as an example. We cannot, however, go so far with Dr. Shufeldt as to look forward to a time when museums shall display in monster cases, picturesquely arranged, the faunas of entire regions or the animal and plant life of various geographical areas. Such a method of exhibition leads, almost certainly, to hopeless incongruities, and prevents a proper inspection of the specimens exhibited. The scene-painter must not interfere with the scientist. A museum is a palace of truth before it is a palace of art.

It is particularly interesting to note the enormous development of the art of taxidermy in the United States which characterises a quite recent period. This is due, in Dr. Shufeldt's opinion, to the stimulating influence that the World's Columbian Exhibition had upon every art and industry, an influence, one may add, that extended far beyond the limits of the great republic. It was not, however, merely the desire to rival other institutions and countries that gave so great an impetus to the art, but the fact that a sufficient appropriation of the needful dollars enabled their true strength and best work to be put forth by such men as F. W. A. Lucas, Joseph and William Palmer, Nelson R. Wood, Henry Denslow, A. H. Baldwin, George Marshall, A. Z. Shindler, and J. E. Benedict, who form part of the enthusiastic staff at the National Museum of the United States of America.

NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 31. Vol. V. SEPTEMBER, 1894.

NOTES AND COMMENTS.

Organic Life on Mars.

THE hold taken upon the popular mind by the astonishing revelations of astronomy is again in evidence. The planet Mars is the nearest neighbour to the earth, and at present the two are at the nearest parts of their circling courses. From the observatory at Nice, Mr. Javelle has seen on the southern edge of Mars a luminous projection. In accordance with the admirable co-operation among the astronomers all over the world, this interesting observation has been telegraphed from station to station. These telegrams naturally have found their way to the public Press, and from their reaction upon the journalistic mind have come a crop of articles and interviews, of speculations and theories, and all these turn upon the twin questions —is the planet Mars inhabited? and are the inhabitants signalling to us?

We share with everyone else the compelling fascination of these questions. Time was when the earth, like a little child, thought itself the centre of all things: now it has grown up enough to know that it is the tiniest unit in the ordered polity of the universe. It is an insignificant appanage of a system which is the merest dot in star-sown space. In the effort to reach some cognisance of our place in the unknown, the planet Mars seems to offer some hope of a standard of comparison, some basis for induction. With a knowledge only of this earth, the imagination may run riot and people the planets of this and other systems with benign or evil figments of fancy. With a knowledge, however slight, of the actual state of things on the surface of Mars, we should lose the morbid fictions engendered by isolation and at the least gain a method by which speculation might be guided.

It is now known that the physical conditions here and on Mars

are similar in many respects. Both the earth and Mars "turn alternate cheeks to the reverberation of the same blazing world," and both encounter the same, or nearly the same, range of temperature. Mars, like the earth, is covered by land and by sea. It has mountain ranges and volcanoes, plains and valleys; it has wind storms and rain and clouds. The polar regions are covered with snow and ice. Summer and winter succeed each other as here, although the actual transition from season to season is much more sudden. Mr. Norman Lockyer, for instance, says that he has seen a tract of snow, that would reach from London to Madrid, melt in a week, and suggests that the so-called canals of Mars are deep channels hollowed out by the sudden periodical melting of the snows. We know, of course, that the same chemical elements, and many of the compounds with which we are familiar here, exist on Mars. The only known physical condition differing much is the force of gravity, which is considerably less on Mars.

Can we conclude anything from these data about the presence of organic life? In the first place, organic life, as we know it, is associated invariably with protoplasm. In that mixture of materials, the chief chemical compounds present are the albuminous substances known as proteids. A temperature about freezing point arrests the activity of these: a temperature above 40 degrees centigrade destroys them. So far there is no difficulty, for certainly there are on Mars, as here, some parts where the temperature remains all the year round within these limits. Next, water must be present, and in this respect also, the conditions on Mars almost certainly are favourable. The water must be tolerably pure: the presence of a certain amount of salts would be no barrier, for, although even the best elementary text-books of biology know only the amœbæ of fresh water, amœbæ are common animals of brackish water, and of the sea-shore. The presence of free mineral acids in solution would be fatal: but from the similar physical and chemical conditions here and on Mars, there is every reason to believe that free acids would exist as seldom in nature there as here. The different conditions of gravity afford more perplexing problems. Oscar Hertwig and others, by experiments upon developing ova in centrifugal machines, and so forth, have shown that neutralising the effects of gravity makes little difference to the forces of growth. But the researches of Bütschli open up graver questions. If there be truth in his analogy between protoplasm and physical foams, only those who have attempted to repeat his experiments can judge how small are the differences in tensions and consistencies that make or mar the results. If the structures and movements of protoplasm depend, as he thinks, chiefly upon differences of surface tension, no one can say how a difference in the force of gravity would interfere with the delicate interaction of forces upon which tensions depend.

But granted the possibility of the mere existence of protoplasm

on Mars, a much wider set of problems at once arises. For we are more interested in the possibility of the existence of that reasoning intelligence which, within our experience, is always associated with such integrations of protoplasmic structures as we find in man and the higher animals. The framework and structure of all animals and plants are adapted so intimately to the existing conditions of gravity, that the course of the evolution of life under other conditions must elude even our imagination. If from the same simple beginnings a chain of life has grown up upon Mars, it must have grown up so different that to us its final results would be a horror and a monstrous prodigy. Even if it has grown so that its highest result has like us become able to be conscious of a side or part of the absolute truth of things, it is very improbable that this consciousness, acting in terms of and through different sense organs, possibly upon a different side of the absolute truth, could ever come into relation with ours. If ever we could "open up communication with Mars" in the sense of projecting such physical signs as would be intelligible to minds acting through sense organs like our own, it is more than probable that they would be unintelligible to minds deaf to what we hear, blind to what we see, not feeling what we touch, and realising their environment in ways inscrutable to us.

Lord Salisbury's Address.

The University of Oxford and the English nation have equal reason to be proud of the remarkable address of the President of the British Association; for, so far as the University is concerned, their supreme officer, the Chancellor, is chosen for very different reasons than his eminence in science; so far as the nation is concerned, Lord Salisbury is honoured as a man of affairs, as one who gained worthily and held worthily the great office of her Chief Minister. It is the fashion to say that the University is the home of effete culture; it is the fashion to say that politics are in the hands of mere rhetoricians, of shallow devotees of expediency. Here is the head of a great University, an acknowledged leader of politicians, who can hold enthralled on scientific subjects an audience as full of scientific experts as ever is gathered together. Discount all one may for the modulations of the practised speaker, for the political habit of exquisite adaptation to an audience, it remains to be said that unless Lord Salisbury had had solid scientific matter in his speech, as well as subtle intellectual form, he would not have received the enthusiastic reception that was his.

It is easy to analyse his arguments and to criticise his conclusions. To these agreeable tasks we shall presently address ourselves. It is not so easy, on after thinking, to account for the general scientific approval of an address which was really an exposure of what the

average cultured Briton regards as the failures of science. Probably the root of the matter is this. With regard to any scientific subjects but their own, scientific men are no other than average cultured Britons. During our conduct of Natural Science this has been thrust upon our notice. It is our endeavour to have subjects so treated that the results of any research shall be intelligible to those who are unfamiliar with the methods of that research. Of course we receive much criticism, but, for the most part, it is not the average cultured Briton who writes to say that a paper on biology, or on geology, is unintelligible. It is the working geologist, apparently, whose standard of the intelligible in biology is the lowest; it is those who are adding to our knowledge of plants and animals that find most difficulty in comprehending what geologists say they are about. Lord Salisbury's address dealt with three or four widely-different branches of science, and we may take it that although his audience included a large number of experts in various branches, yet the verdict of the audience was not the verdict of science, but the verdict of the average cultured Briton. Why, then, is average culture so cheered by the exposition of "the great failures" of science?

An Unfounded Story.

It is reported, in an entirely unveracious chronicle, that after the discovery of America, Columbus returned to Spain and settled down to a life of dignified ease. He became the lion of several seasons, and, as the great discoverer of America, was the favourite of the Court and the darling of the populace. He was invested with every national and civic dignity, and grew a power in the land. When a question of state was being discussed in the council of the king, the king himself would command that the voice of the discoverer be heard. When the populace was seething and troubled, and demagogues shouted against the accredited rulers of the nation, the words of the discoverer of America acted like a charm. But one day, in a moment of expansion, Columbus remarked that there were, no doubt, many undiscovered countries, and that, personally, he had a longing to discover the North Pole. His words were taken up and repeated upon all sides, and dissatisfaction grew with their repetition. Finally, the king himself sent for Columbus, and said:—"Columbus, we honour you greatly and have rewarded you, perhaps lavishly, for your discovery of America; but we must remind you that you have not discovered the North Pole. There is but one America; there are, you tell us, two Poles. We will not bind you harshly to the discovery of either or both. There are two Poles—discover us one of them." And so Columbus, gathering an expedition, set out for the north. When, after years of wandering, he returned, unsuccessfully, he sank into an unhonoured grave, known not as the great discoverer of America, but as the man who failed to find the North Pole.

Lord Salisbury's North Poles of Science.

Two of these undiscovered countries of science (the nature and origin of the elements and the nature of ether) are outside the special scope of Natural Science. But in passing we may observe that, while modern science is as unable to transmute the metals as were the early alchemists, it is the merest dogmatism to say that the boundary of our knowledge in this direction remains where it was many centuries ago. The cataloguing and recording of the elements, the negative results themselves of Stas, the researches of Crookes upon the evolution of the elements, and the grouping of them by Mendeléeff, are all great strides in knowledge. We may be near or far from knowledge of the nature and origin of the elements: it is certainly true that we are nearer the goal by those years of brilliant result and patient research upon which Lord Salisbury justly laid so much stress.

With regard to the nature and origin of life, we should reply to Lord Salisbury in similar fashion. It is quite true that we do not know the exact nature or the origin of life. But we do not regard it as a problem so remote as to rank among the almost impossible quests of science. If to-morrow Professor Bütschli or some other investigator were to demonstrate that the phenomena of life could be called into existence in the laboratory, we should feel that an advance much more momentous in the minds of the populace than in the minds of biologists had been made. For the result of recent research has been to break down the barrier between the mystery of living matter and force and the mystery of inorganic matter and force. Biologists will not accept Lord Salisbury's definition of life as "the action of an unknown force on ordinary matter," as "a mysterious impulse which is able to strike across the ordinary laws of matter, and twist them for a moment from their path." Whatever be the nature of life, it is not a force twisting and thwarting the ordinary laws of matter. Nor is life in any ordinary sense of the term a force at all. If we speak of the chemical or physical work done by living organisms as the results of the force of life, either we use the term as the vaguest metaphor, or we imply that the force of life is only a phase of ordinary physical and chemical forces. For the work done by an organism can be measured in terms of the chemical and physical energies it employs, and the actual growth of an organism, the multiplication of cells, depends upon ponderable transformation of ordinary physical and chemical forces. The results of much recent work on the nature and functions of protoplasm lead many to think that the supposed barrier, between living matter and matter that is not alive, has already been broken down, and that living material is only a peculiarly complicated mixture of known chemical and physical forces. Be that as it may, there is more of the orator than of the thinker in Lord Salisbury's handling of the problem of life. Life, like electricity, or

chemical affinity, or gravity, is not a thing to be explained in a formula. We know the positive and negative conditions of its existence; every month adds to our cataloguing of its phenomena and of its relations to other phenomena. We know nothing but by its qualities, and it is a fair statement to make that science has learned to know as much of the qualities of life as it knows of the qualities of anything. As each step in the slow accumulation of knowledge is made, it will always be open for someone to say, "Ah, my friends, you have discovered America, but how about the North Pole?"

Lord Salisbury and Natural Selection.

Darwinism and the Doctrine of Descent are now in the unhappy position of popularity. Only those who have some practical acquaintance with the facts involved, with the anatomy and embryology of living animals, and with the history of their fossil remains in the rocks, hesitate to lay down the law for or against what they consider Darwinism. Lord Salisbury's acute mind has avoided the most vulgar error. Since Darwin's solution laid hold of the intelligence of the world and armed it with a new method for the study of organisms, month by month proofs of the doctrine of descent have accumulated, until now no naturalist of position or attainments asserts the immutability of species. But we go beyond Lord Salisbury, and say confidently that there are not six among the well-known biologists of the world who do not hold it as proved that all animals and plants that are alive, or have lived on the earth, are connected with each other by the chain of a common descent. It is upon this achievement that the undying fame of Darwin is based. The dispute about the fact of evolution is over. After the lapse of so many stormy years, the word evolution has earned its right to Lord Salisbury's witty designation "comfortable." It is, as Lord Salisbury rightly said, concerning the method or mechanism of evolution that there is still controversy, and that there may be controversy to the end of time. At the end of the introduction to the first edition of the "Origin of Species," Darwin wrote:—"I am convinced that Natural Selection has been the main but not the exclusive means of modification." At different times after the publication of the "Origin," as may be seen in the different editions of that and other works, Darwin was inclined to attribute sometimes a greater, sometimes a less, important sphere of influence to this factor. At the present time one school, of which Weismann is the leading exponent, believes that Natural Selection, to the exclusion of other factors, is the motive force in evolution. Probably a majority of living naturalists attach to it less importance than did Darwin. But it was because Natural Selection or, "the preservation of favoured races in the struggle for life," furnished an intelligible mechanism for the process of evolution, that Darwin was able to

convince the scientific world of the fact of evolution. Whether or no the final verdict of science be for or against Natural Selection, it will remain the most stupendous example of the value of scientific theory in directing scientific observation.

But Lord Salisbury has regarnished for us two objections to the validity of Natural Selection. The first of these is the question of the exact amount of time during which evolution has proceeded. Now we do not know the duration of time which is expressed by the Tertiary strata. We do know that, relatively to preceding periods, the Tertiary period is but a short episode in the history of the earth. Yet, in this short period, there have come into existence nearly all the species and genera, and many of the families now living. A very small acquaintance with geology shows that, whatever be the case with the method, there has been time for the actual occurrence, for the excellent reason that the occurrence is actual. As for the amount of time being prohibitive of the process of Natural Selection, Lord Salisbury's objections depend upon a view of the mode of operation of Natural Selection which, so far as we know, is peculiar to Lord Salisbury. But it is not necessary to enter into the view of Natural Selection that the President propounded, because his second objection to Natural Selection is, that we cannot demonstrate the process of Natural Selection in detail, and that we cannot even imagine it. This second and final objection is taken by Lord Salisbury from a recent paper of Weismann. To enter into it would be to open up the whole Darwinian controversy. But we may say that we agree with Lord Salisbury that, if Natural Selection implied that for every step "two individuals of opposite sexes, accidentally blessed with the same advantageous variation, had to meet and transmit by inheritance that variation to their successors," then no lapse of time would be sufficient to transform even one species into another. But that, of course, is a burlesque of the supposed process.

We do not agree with Weismann's statement, quoted with approval by Lord Salisbury, that, if Natural Selection be rejected, there is no resource but to fall back on the mediate, or immediate, agency of a principle of design. But we admit fully that if there be no hypothesis to account for the evolution of species but the action of selection upon indefinite variations in each generation, then there is room, and more than room, for the hypothesis of some inherent and directing force of phylogenetic development, which is simply design writ small. On the other hand, every recent publication bearing upon these matters is full of suggestion and argument and fact against the nature and use of variation being such as Lord Salisbury supposes.

Dwarfing by Experiment.

Those of our readers who have paid attention to the recent researches on the manner and causes of variation, to which we have

of late directed their attention, will be interested in a paper that Mr. H. de Varigny has just published in the *Journal de l'Anatomie et de la Physiologie* (1894, pp. 147–188).

In 1874 Professor Semper published the results of some experiments made with specimens of *Limnæa stagnalis*, in order to ascertain the effects of the environment on the growth of the animal, and more especially the causes of dwarfing. By isolating young individuals obtained from the same mass of ova, and placing them in vessels each containing a different amount of water, he found that reduction in the bulk of water, below a certain quantity favourable to full development, was attended by a corresponding reduction in the size of the animals. Since the young snails were fully supplied with food, and placed under similar conditions of temperature and light, and since the state of aëration of the water would be without effect on these air-breathers, the cause of the dwarfing was not very apparent; and Dr. Semper was finally driven to conclude that the reason must be sought in the presence in the water of some substance, not as yet ascertained, which was necessary to the growth of the animal, and the amount of which in the smaller volumes of water was insufficient.

To test these observations more fully, and more especially the Professor's conclusion, Mr. de Varigny undertook a series of experiments. Some of the results arrived at he has embodied in the paper before us. He selected as subjects *Limnæa stagnalis* and a species which he terms *L. auricularis*, but which from his figures is undoubtedly an inflated form of *L. peregra*. Individuals hatched from the same mass of ova were isolated in vessels containing different amounts of water and offering equal or different areas of aëration in the several experiments. The vessels were placed under equal and favourable conditions of temperature and light, and the animals furnished with abundant food. The results went to show, first, as Semper's trials did, that, within limits, the size of the shell increased with the increase of volume of the water the animal was placed in; secondly, that the dimensions of the *Limnæa* increased in proportion to the increase of surface exposed to the air, and that increase was almost more pronounced than in the case of increase of volume of the water alone, and even persisted when the superfices was increased while the volume was diminished; thirdly, other conditions being equal, the size of the shells varied in inverse proportion to the number of individuals inhaling a given volume of water. Semper's conclusion, however, as to dwarfing being due to some unknown substance in the water, was neatly disposed of by experiments in which individual *Limnææ* were penned off from their fellows, and compelled to occupy a small space in the same vessel, while a thorough circulation of the water was effected. The results were just the same as if they had been confined in separate small receptacles.

Seeking, therefore, to explain the cause of this dwarfing in some other way, Mr. de Varigny suggests that it is due to lack of room in

which to move about, a proposition he supports by ingenious arguments, while admitting that further experiments (now actually in progress) are necessary. While looking forward to the publication of these further details, we would venture to commend this paper to the special attention of certain of Mr. de Varigny's countrymen and others in this country, as offering, by means of its figures, a magnificent opportunity for self-advertisement in the creation and naming of limitless varieties and so-called new species.

"A Moving Grove," *Shakespeare.*

In our June number (vol. iv., p. 406) we had something to say about the way in which animals were protected from discovery by their resemblance in colour or form to surrounding objects. Now a man, if he wishes to escape observation in this manner, does not change his shape or the colour of his skin, but clothes himself in some appropriate substance, or, like the soldiers of Macduff when they advanced on Dunsinane, hides himself beneath a covering of natural objects. Just in the same way, though possibly without the same conscious intent, certain of the very humble dwellers in our modern seas take to themselves a cloak fashioned from the common objects of the sea-floor. Thus, a certain coiled shell has received its name, *Xenophora*, "stranger-bearer," from this very habit; for the molluscs of this genus attach to their shells, which are usually very thin, such things as stones, shells and pieces of coral, which are found at the bottom of the sea in which they dwell. Three such specimens have recently been placed in the central hall of the Natural History branch of the British Museum, one of them, *Xenophora pallidula*, from Japan, and the others, *X. conchyliophora*, from Bermuda. One of them is covered with a number of other shells, and another with stones and sand; and it is interesting to see how large these foreign bodies are in proportion to the fragile shell itself, and how completely they hide it. Close by them there is exhibited a crab, *Maia squinado*, which came from Plymouth, and is entirely covered by fragments of seaweed, sea-mats and sea-firs. These organisms are not merely growing on the shell of the crab, but are attached to it by the hooked hairs with which the exposed surface of its body and limbs is clothed.

These cases are interesting enough, but a further and more wonderful development of the same method of protection is found in certain sea-urchins that inhabit the harbour of Kingston in Jamaica. Their names are *Toxopneustes variegatus* and *Hipponoë esculenta*, and unlike many other sea-urchins or sea-eggs, as they are called in Jamaica, these species do not stay at home in holes or crevices, but wander about the smooth, hard bottom, in very shallow water, where many sea-weeds are growing. But like other sea-urchins, these animals are provided with little suckers or tube-feet, and while they use some of these to drag themselves along, they use the suckers on their backs

to carry about bits of sea-weed and shell and small pebbles. By this means they very successfully conceal themselves from the naturalist and no doubt from other enemies; but the edible *Hipponoë* is, unfortunately for itself, less an adept at the practice than is the species of *Toxopneustes*.

Nematodes in the Blood of Man.

The English race, among the many interesting things which they bring back from the tropics, occasionally unwillingly bring back for the consideration of English medical men parasites of the tropics. In the new issue of the *Transactions* of the Norfolk and Norwich Field Naturalists' Society Mr. H. D. Geldard describes an interesting case of *Filaria sanguinis hominis* in the blood of a patient who resided many years in India. The specimens abounded in the blood of this patient, and were that larval phase of *Filaria bancrofti* which is called *Filaria nocturna*, as it appears in the blood of man only from sunset to sunrise. These young embryos are taken from the blood by the female mosquito, which also is a nocturnal animal. A few days after its meal the mosquito retires to water to deposit her eggs, after which she dies, and her dead body drops into the pond or stream. Within her body the embryos which had got into the alimentary canal from the blood of man have been rapidly maturing. They issue from her dead body by means of a boring apparatus developed at their anterior end. They swim freely through the water, and apparently ultimately perish unless they are swallowed in the water by man. If this happens they bore their way from the stomach to the lymphatic vessels, where they become sexually mature. Each female pours out a multitude of small embryos, each within a delicate egg-shell. These find their way into the blood-vessels and lie concealed, except at night, when the mosquito is active, and so gives the opportunity for this cycle of development to begin again. The consequences of their presence in man may be trifling, but by choking the lymphatics they may cause some most unpleasant and dangerous diseases.

Hop-Disease in Kent.

We learn from Professor J. Percival, of the South-Eastern Agricultural College, Wye, Kent, that he is at present engaged investigating the disease which causes hops to become what is known as "nettle-headed." The disease is met with in many districts in the county, and has rapidly increased during the last few years—in some cases leading to a complete destruction of considerable areas of hop gardens. The early delicate varieties are most attacked, and especially those which have been growing for a considerable time. It is rarely until five years after planting that the disease is seen, and usually not until a much later period.

In a typically affected plant the leaves are smaller than is generally the case, and are transformed in such a manner as to become strikingly like those of the stinging-nettle. The edges of the leaves turn upwards; the fibro-vascular bundles in them are increased slightly in thickness, and stand out well from the under surface of the leaf.

Closer examination shows destruction of the soft tissue in immediate contact with the fibro-vascular bundles; the parts become thin and yellow, and much resemble the thin spots produced by the action of sulphur dioxide gas on the leaf.

The internodes of the plant are short and the main stem and branches taper more rapidly than in healthy plants. In practically all cases the bine or stem, after climbing a short distance up the pole, loses it power of twining and topples over; when not tied up the whole stem becomes slack, slides down its support, and the plant lies in a heap on the ground.

So far the experiments, which are being carried out on the Bettring estate of Messrs. E. A. White & Co., Paddock Wood, point to the circulation within the plant of a poison which brings about curling of the leaf, stoppage of growth of the fibro-vascular tissue, and loss of turgor of the cells in the leaf near the ribs, with resulting yellow patches there.

The leaves on the main stem nearest the ground are affected first, and then follow those at the nodes above in regular succession. The branches in the axils of these show the same regular sequence of diseased leaves, the lower branches being affected first. The cause is undoubtedly connected with the root, and Professor Percival finds in the cortex of many diseased specimens, close up to the bast, considerable numbers of the nematode *Tylenchus devastatrix*, Kühn, usually known as an eel-worm.

Before any completely satisfactory cause can be established or remedy suggested, much work is necessary. At a later date we hope to be able to give an extensive communication from Professor Percival dealing with this interesting and important investigation.

The Indian Geological Survey.

The usefulness of the Geological Survey of India appears to be fully recognised by the practical men of the Dependency. Thus, we read in the *Records*, vol. xxvii., p. 65, that, in his inspection of the Bengal collieries, Mr. Grundy, the Inspector of Mines, has "experienced very earnest co-operation on the part of the agents and managers of the mines." On the other hand the Survey is not blind to the advantages it may itself gain by friendly co-operation with the practical men. The maps of many large coal-fields that were made some twenty and thirty years ago, on the largest scale available at that time, are now found to be insufficient. It is suggested therefore

that the Bengal Colliery managers should form a committee to prepare a revised and enlarged map of the Ranigunj coal-field. "The managers would appear to have quite sufficient data before them in the way of borings and shaft sections, which, combined with their local knowledge, should enable them to contribute materially to the production of a lasting standard work of this kind. An initiatory step might be made by supplying managers with copies of the latest revenue and other maps of their districts, on which each gentleman could enter all the data at his disposal according to some settled scheme of geological and mining delineation. The Geological Survey Staff is all too limited for such an undertaking, but by judicious co-operation with a committee of the kind indicated, a great deal might be done to effect the desired end." A concrete instance of the assistance that may be so rendered is already to hand in an excellent map of the Giridh coal-field by Dr. Walter Saise, manager of the East Indian Railway Collieries, which is to be presented to the Survey for publication. It is distinctly cheering to find that the Indian Geological Survey does not snub the work of private observers in the way that some other Government Departments are accused of doing.

Tertiary Rivers.

The little island of Eigg in the Inner Hebrides has long had a fascination for the geologist. Readers of Hugh Miller's charming book "The Cruise of the Betsey" will remember his description of the Scuir of Eigg, that large wall of curious pitchstone-porphyry that surmounts the southern end of the island, running across it from E. to W. This wall appears to be the remains of a once far larger mass of lava, which flowed down from some adjacent volcano and filled a river valley. The river deposits of this valley, with remains of a pine, are preserved beneath the Scuir. At the meeting of the British Association, Sir Archibald Geikie drew attention to a little rock called Hysgeir, which lies in the sea some eighteen miles west of Eigg. This rock consists of just the same peculiar porphyry as the Scuir, but lies at a much lower level. It therefore seems to prove a further extension of the river-valley, and a descent of it about 35 feet in a mile. Some other islets north of Hysgeir exhibit records of a still older river. In what must have been the valley of this river there are found a succession of coarse river gravels alternating with bedded basalts. The lowest of these gravels contains remarkably large blocks, some as much as six feet in length. Some of the gravels or conglomerates pass into true volcanic agglomerates; while in other places there are layers of fine tuff, volcanic sandstone, or shales with remains of land-plants.

The sequence of events which these various deposits indicates was described by Sir Archibald as follows. "During the outpouring

of the lavas of the great basaltic plateaux of the Inner Hebrides, a river flowed across the volcanic plain from the Western Highlands, whence it carried large quantities of shingle. By successive violent floods, these materials, together with the detritus of the lava-fields, were strewn irregularly far and wide beyond the immediate channel of the river. In the pools thus left behind, fine volcanic silt gathered and entombed leaves and stems of the surrounding terrestrial vegetation. But volcanic activity still continued, and though ashes, slags and pumice were swept down, new eruptions took place by which masses of rock, sometimes nine feet in diameter, were thrown out to a distance of a mile or more, and fresh streams of lava were poured out, completely burying the previous accumulations. Renewed river-floods of gradually-lessening severity spread fine detritus over the cooled sheets of basalt, and again these later fluviatile deposits were entombed between fresh outbursts of lava. Perhaps no more striking evidence can be elsewhere obtained of the condition of the land surface over which, from many scattered vents, the materials of the volcanic plateaux of the Inner Hebrides were slowly piled up." It is interesting to find that in these far-distant Tertiary times the slope of the drainage areas of the Western Highlands was the same as in our own day.

Landscape Marble or Cotham Stone.

The curious deposit known as Cotham Stone has exercised the ingenuity of many to find a cause for its formation. As early as 1754, Edward Owen published a work entitled "Observations on the Earths, Rocks, Stones and Minerals for some miles about Bristol, and on the Nature of the Hot Well and the Virtues of the Waters," and in this book he suggests that the peculiar arborescent structures of the Cotham Stone were produced by the escape of imprisoned air, which permitted the blacker portions of the mud to follow the bubbles, and so get dispersed in tree-like patches. Others have thought that gaseous emanations from the *Avicula contorta*-shales had produced the peculiar markings, and others again suggested that mineral infiltrations would account for them.

Mr. H. B. Woodward in 1893 (Mem. Geol. Surv. The Jurassic Rocks, vol. iii., p. 31) gave it as his opinion that the "arborescent markings were produced during the consolidation of the stone, and more particularly by the shrinking of its upper portions. In this way, and while the mud was still in a more or less pasty condition, one or more of the dark films in the banded mass were disarranged and dispersed in arborescent form in the slowly-setting rock." He suggested also that pauses in deposition and exposure to the sun's rays might have accounted for some of the structure, and concluded that markings were due mainly to mechanical forces, though there were evidences of chemical change.

Mr. Beeby Thompson, in the last number of the *Quarterly Journal of the Geological Society* (August, 1894), has examined all previous writings on the subject, investigated the matter, and concludes that the peculiar characteristics are due to interbedded layers of vegetable matter, which continued to decompose and evolve carbonic acid gas and marsh gas after deposition; and that where a layer of extra thickness occurred, the decomposition continued whilst a thickness of several inches of new sediment was laid down, with the result that arborescent markings were produced along the lines taken by the escaping bubbles. Mr. Thompson, in this interesting paper, gives his reasons for these conclusions, and records his experiments to reproduce artificially the characteristics of landscape marble.

The Systematic Position of Trilobites.

For many years the position of the Trilobites in the scale of organised beings has been doubtful. One school has insisted that they should be referred to the Crustacea, and another to the Arachnida. Mr. H. M. Bernard has just published, in the *Quarterly Journal of the Geological Society* (Aug., 1894), an important zoological paper in which he discusses at length the question, and refers these ancient life-forms definitely to the Crustacea, the original position assigned to them by Linnæus and by Burmeister. We are unable to give the details of Bernard's paper, but quote his own words as given in his "summary":—

"It is now possible . . . . to fix with great probability the zoological position of the Trilobites. The bending round ventrally of the first segment, the great labrum with antennæ attached at its sides, the 'wandering' of the eyes, the pores (pointing to the probable presence of water-sacs), the head with a varying and progressively increasing number of segments, the dorsal organ, the rudimentary character of the posterior segments, and the gradual diminution in size, with the essentially lobate or phyllopodan type of the limbs, all serve to connect the Trilobites with *Apus*.

"This relationship cannot, however, be considered as direct. *Apus*, on account of its richer segmentation, the absence of pleuræ on the trunk-segments, and its more membranous parapodia-like limbs, must be assumed to lie in the direct line upwards from the original annelidan ancestor towards the modern crustacea. The Trilobites then must have branched off laterally from this line either once or more than once, in times anterior to the primitive *Apus*, as forms specialised for creeping under the protection of a hard imbricated carapace. This carapace was obtained by the repetition, on the trunk-segments, of the head-shield which, as we have already seen, almost certainly existed as a structure *sui generis* in earlier forms, and, somewhat modified, has been retained as such in the early crustacea proper ('Aspidophora').

"It seems to me, therefore, that the Trilobites, studied in the light of new discoveries, especially of those which we owe to American investigators, yield the most interesting and important evidence as to

the origin of the crustacea. Stripped of their pleuræ and of the expansion of the head-shield, we have, in the early Trilobites (*e.g.*, *Olenellus*), long segmented animals tapering at the posterior end. The first segment is bent round ventrally, so that the large labrum points backwards. The appendages of the first segment appear to have functioned as sensory organs and to have pointed downwards, being inserted at the sides of the labrum. The following segments were provided with membranous lobate appendages carrying, on their dorsal edges, gills and sensory cirri, and distally specialised into locomotory organs. The alimentary canal ran through the whole length of the body, bending round anteriorly to open through the mouth.

"The Trilobites may thus be described as *fixed specialised stages in the evolution of the crustacea from an annelidan ancestor, which bent its mouth round ventrally so as to use its parapodia as jaws*."

To us it has seemed in the highest degree improbable that crustacea could have come from a specialised annelidan, but it will be interesting to see what the "arachnoid" theory-holders have to say in reply to this paper, which appears to be carefully and thoughtfully worked out. It is fully illustrated, and marks a distinct revival of palæozoological interest in the Geological Society which we are pleased to see. We note, however, that of those four who entered into the discussion of the paper, three were zoologists, geologists as a rule caring for little else than stratigraphy or petrology.

Species-Making and Species-Taking.

In one branch of biology there still rages a battle about species. Those who are concerned with the collection, naming, and description of fossil and living species are troubled constantly by the stupid and unprincipled action of species-mongers of the baser sort.

Opposed though we are to too great extension of professionalism or officialdom in science, we have sometimes wished that there could be something in the nature of a diploma without which no one should be permitted to practice, *i.e.*, to publish, or that there were some means of disabling an incompetent or dishonest writer, such as the Bar exercises when it strikes a man off the rolls. Even now there is a case that, if the facts be correctly stated, certainly calls for some such drastic treatment. A writer named Victor Lopez Seoane has recently been distributing a couple of pamphlets, entitled "Aves nuevas de Galicia" and "Revision del Catalogo de las Aves de Andalucia," both of which bear the imprint "La Coruña, 1870." There is, however, clear internal evidence that the date of one of these is incorrect, as it refers to the British Museum Catalogue of Birds, the first volume of which did not appear till 1874. Moreover, it is stated by Mr. C. D. Sherborn, who discusses the question in the August number of the *Annals and Magazine of Natural History*, that when these pamphlets reached him "the ink in which they were printed was apparently fresh and easily smeared." Whether or no it is designed for the purpose, this pre-dating gains priority for some

of Seoane's own names over names proposed by Howard Saunders and Reichenow. The sooner such writings are ruled out of court, the better it will be for systematic zoology.

Another case of what seems illegitimate "Species-making and Species-taking," is illustrated by a little polemic now proceeding between two well-known palæontologists, Professor Ralph Tate, of Adelaide, and Dr. Bittner, of the Royal and Imperial Geological Institute of Vienna. Every now and again some "lumping palæontologist" figures two specimens to represent the same species; then, to his annoyance, one of the "splitting" students of that science promptly makes a new species out of the second figure. We have all heard of cases in which some over-zealous member of the *nouvelle école* of conchology has made a second species out of a different view of the same specimen. Dr. Bittner has not been so unscientific as this. Some time ago Professor Tate sent to Vienna a collection of specimens of the Australian Lower Tertiary Echinoidea. As this fauna has not yet been fully worked out, Professor Tate asserts that he was very careful to send only well-known species, and to select what he regarded as most typical forms. Nevertheless, so different are Dr. Bittner's and Professor Tate's views of specific differences, that the former subsequently issued a paper ("Ueber Echiniden des Tertiärs von Australien." *Sitzber. Ak. Wiss. Wien.*, Bd. ci., Abth. i., pp. 331–371, pls. i.–iv.) in which he founded seven new species and one new variety on this material. That he also established two new genera for two species which had been previously described was not surprising. To this Professor Tate has replied in some "Critical Remarks on A. Bittner's 'Echiniden von Australien'" (*Trans. Roy. Soc. S. Australia*, vol. xv., pp. 190–194), in which he refuses to accept any one of Bittner's new species, and maintains that these are all founded on specimens as typical of the old species as any that could be selected. He ascribes Dr. Bittner's error to his ignorance of the specimens as they occur in the rocks, a cause which is undoubtedly efficient in many other instances of splitting palæontology.

I.

Rev. George Henslow on Natural Selection.

IN NATURAL SCIENCE of July Mr. Henslow makes some statements with regard to variation and Natural Selection which call for critical remark. He says that, though cultivated plants vary *indefinitely*, and therefore require selection to produce definite modifications, this is not the case in Nature—" Variation in Nature is always in strict adaptation to the direct action of the environment; in other words, *natural variation is always definite*." This statement seems to me so extraordinary, and so opposed to well-known facts, that I can only impute it to the use of the terms "vary" and "variation" in two very distinct senses; first, as meaning those individual variations which occur abundantly both in nature and under cultivation; and, secondly, as meaning those particular variations which alone survive under nature and produce a "variety" or a "species." In this latter sense, of course, "natural variation" is definite; but so, in the same sense, are the variations of cultivated plants. From unstable and indefinite "variations" man and nature alike produce definite "varieties." As one out of the innumerable examples of indefinite variation which might be named are the fifteen different *modes* of variation observed by Alph. de Candolle on a single oak tree, while in a great number of common species an equal amount of variability may be observed both in wild and cultivated individuals; and all these variations are *indefinite*, in the sense that they do not usually occur in one direction only, from the typical form. A few examples of such variations have been given in my "Darwinism," pp. 76–80. I cannot, therefore, understand either the meaning or the value of the statement—" *natural variation is always definite*."

It is not quite clear whether Mr. Henslow admits the agency of Natural Selection at all. He says: "I would ask what *facts* are producible to *prove* that Natural Selection acts at all on the maintenance, if not the origin, of any floral and, indeed, other structures?" It is, of course, admitted that *direct* proof of the action of Natural Selection is at present wanting; but the indirect proofs have been so cogent as to overcome the most violent prejudice and opposition, and to convert a large majority of naturalists to a belief in its agency. It is, therefore, rather late in the day to deny its existence without

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adducing some adequate and proved substitute. What Mr. Henslow does put in its place is the reaction of vegetable tissues to the environment, resulting in adaptation; and in the special case of flowers he imputes all the variety of form and endless modifications of structure to "the responsive action of the protoplasm, in consequence of the irritations set up by the weights, pressures, thrusts, tensions, etc., of the insect visitors."[1] Now the very first essential to this theory is to prove that modifications produced by such irritations are hereditary. Here, if anywhere, we want facts. Yet in the very interesting volume to which Mr. Henslow refers us, crowded as it is with facts and observations, I can find only two or three slight references to this most vital point. At page 147 he quotes Darwin as saying that the excellence of our milking cows and goats may be attributed partly to selection and "*partly to the inherited effect of the increased action, through man's art, of the secreting glands*," and adds, "This fact" is strictly analogous to what takes place in the vegetable kingdom! Here we have a mere opinion of Darwin's, nowhere supported by direct observation or experiment, and now seriously challenged by a large body of naturalists, set forth as "a fact." Again, at page 157, the case of the various "ant-plants" of the eastern tropics is referred to, and it is stated that Dr. Beccari explains the curious hollow stem in which the ants dwell as partly due to the irritation of the ants inducing hypertrophy of the vegetable tissue, which "then becomes hereditary"; and Mr. Henslow concludes that there is abundant evidence to prove that many organs of a plant, if subjected to irritation, can become materially altered and develop new processes, and, "secondly, that these altered states, if the irritation be persisted in, may become hereditary." Here again are only opinions without a particle of proof; and I can find nothing more to the point in the whole volume. The case of galls is very briefly referred to at p. 144, and their non-heredity is passed by with the remark that the predisposition to produce them *may* be greater now than formerly, and that the galls themselves *may* be larger than they were at first. But surely if the effects of insect irritation are anywhere hereditary it would be here. An oak tree which lives several hundred years is subject to this irritation in greater or less degree almost every year, and the irritation itself is not momentary and intermittent, as in the case of insects visiting flowers, but is kept up by the presence of the egg and growing larva during a considerable portion of the period of active vegetable growth, and this has been going on for thousands, probably millions, of years. Yet neither do oaks nor any other plants produce galls spontaneously, as they certainly should do if the results of irritations are in any general sense hereditary. This seems to me to be a really crucial experiment continually repeated by nature.

I may here remark that Mr. Henslow's theory utterly breaks down owing to the want of any conceivable connection between

[1] "The Origin of Floral Structures," p. 340.

insect irritation and most of the innumerable adaptations of the parts of flowers to attract insects and secure cross-fertilisation. Such are the sticky glands, the elastic filaments, the springs and traps, and the accurately timed motions of the pollinia in orchids; the innumerable complexities in papilionaceous flowers; the large coloured tracts in Bougainvillea, Poinsettia, and many others; the flowers with tightly-closed lips, as Linaria, Antirrhinum, Melampyrum, etc.; the enlarged rays of Compositæ, Umbelliferæ, and Caprifoliaceæ; the general massing of small flowers into heads, umbels, corymbs, or dense racemes, so as to become conspicuous, and many other characters. To these may be added the negative evidence of the numerous genera and orders of regular flowers, such as Campanula, Rosaceæ, Gentianaceæ, and many others, which, though thoroughly adapted for insect fertilisation, and whose lower petals have therefore been always subject to irritations, have never developed irregular flowers. In all these cases variation with Natural Selection will account for the phenomena, while insect irritations, even if we admit heredity, will not do so. From whatever point of view we approach the question, the attempt to explain floral structure and colour without the aid of Natural Selection is a hopeless failure.

In the *Journal of the Linnean Society* ("Botany," no. 208, July 10) there has just appeared an elaborate paper by Mr. Henslow on "The Origin of Plant-Structures by Self-Adaptation to the Environment, exemplified by Desert or Xerophilous Plants," in which the author still further develops his view as to the influence of the direct action of the environment unaided by selection. The only portion of this paper on which I propose to remark is that dealing with the origin of spines and prickles, on which I have already had occasion to write in my book on "Darwinism," when combating Professor Geddes' views on the same subject. Mr. Henslow imputes the spines and prickles of so many plants inhabiting dry countries to the direct influence of the conditions under which they live. This, he thinks, is proved by some of these plants losing their spines when grown under other conditions; he adduces numerous examples of the abundance of spiny plants in such countries as Nubia, Abyssinia, and the Kalahari Desert; and he again and again reiterates the statement that these characters are "simply the inevitable results of the action of environment."

Now if these statements comprised all the facts, that is, if in all dry countries spiny plants abounded, while in all moist or fertile districts they were absent or very rare, the explanation given of their origin would have some plausibility. But there is no such general coincidence of aridity of soil or atmosphere with abundance of spiny plants, as very little enquiry will show. Mr. Henslow points out several other plant-characteristics which indicate, and, as he thinks, are directly caused by, aridity. Such are very small, coriaceous, or rolled up leaves, or their complete absence; a hairy or woolly covering

to the whole plant; succulent foliage; special protection of the buds; enormous development of roots; abundance of bulbs and tubers; together with thickness of bark and various protective coatings to stems and leaves. Now many of these peculiarities are present in the flora of the Brazilian Campos—as well described in the memoir of Eug. Warming on Lagoa Santa—which is referred to by Mr. Henslow as corresponding in many respects with that of other arid regions. Yet the author of this memoir expressly states that "spiny plants are very rare" (p. 463). Again, the plants of the Galapagos present similar indications of aridity—shrubs with minute and almost invisible leaves, for example—yet, except the cacti, which may be of American origin, none of the endemic species are spiny. So, also, the rich Sandwich Island flora contains hardly a single endemic spiny plant; and I am informed by the Rev. R. P. Murray, who is well acquainted with the botany of the Canaries, that spiny plants are exceedingly rare in those islands, though much of the surface, owing to the porous volcanic rock and the long periods of drought, presents the conditions which elsewhere are said to produce spines.

Now without denying that—other conditions being equal—aridity may favour and moisture may check the growth of spines, there is another and altogether different set of conditions which seem more directly connected with their abundance or rarity. This is, the presence or absence of herbivorous mammals, against whose ravages spines are a protection. The most destructive of these animals are camels, goats, and antelopes, and it is where these are indigenous—in Arabia, North-east and South Africa, and Central Asia, that thorny shrubs and trees are especially abundant. Again, few countries have more spiny plants than Chili, where the camel-like vicuguas and alpacas, as well as large rodents, are very destructive. But the country is not especially arid, and the remarkable Puyas, whose leaves are armed with excessively sharp recurved spines, inhabit the subalpine regions where rain and mist prevail. In our own moist islands we have a full proportion of prickly plants, and the same may be said of North America, where the *Gleditschia* or Honey Locust has the young branches, and in old trees the trunk, armed with groups of very strong and sharp spines. So also in Japan, notwithstanding its moist insular climate, we have an *Olea* and an *Osmanthus* with holly-like prickly leaves; while the prickly *Berberis Darwinii* is found in the damp atmosphere of the Straits of Magellan.

Equally opposed to the theory of aridity as the efficient cause of spines is their abundance on palms growing in the hottest and moistest regions of the globe. In many Amazonian species the stem is thickly set with long and very sharp spines pointing downwards, and thus forming a complete protection against monkeys and other arboreal fruit-eating mammals. Many species of *Bactris* and *Astrocaryum* are thus armed, as is also the beautiful *Guilielma speciosa*, the Peach palm, whose fruit is large and edible. It is a suggestive

circumstance that, with the exception of palms, few large trees are spiny, and when they are so, as in the case of the *Gleditschia*, the spines are most abundant on the trunk and on the younger branches. In the same way, our holly, when it grows to a large size, usually has the leaves towards the top spineless: the wild pear also is spiny below but unarmed above. The climbing palms, on the other hand, are armed to the very top, but in this case the spines assist climbing.

The anomaly of the flora of the Brazilian Campos having most of the true xerophilous characteristics, yet being almost wholly without spiny forms, is quite in harmony with the fact of the great poverty of this region in mammals destructive of woody vegetation. There are really none but a few deer and cavies, which are mostly inhabitants of the more wooded valleys, and which are kept from undue multiplication by the considerable number of species of Felidæ and Canidæ in the same area.

We are, therefore, led to conclude that the apparent direct dependence of an unusually spinescent vegetation on arid conditions of soil or climate is to a great extent deceptive. Such conditions are inimical to the growth of dense forest, and it is a well-known fact that the larger mammalia abound most in partially wooded or open country. Many of these animals are exceedingly destructive to shrubby or aborescent vegetation, especially in districts which are subject to occasional droughts; and it is in such areas that so many of these plants have acquired the protective armature of spines or prickles, while others not so protected have sooner or later succumbed, thus leading to a preponderance of the former. But the numerous instances in which considerable areas and extensive floras are found to have hardly any spinous plants, as compared with other areas in which the soil and climate are generally similar and where such plants abound—the only important difference being the absence or presence of destructive herbivorous or frugivorous mammals—show us clearly that it is the latter rather than the former condition which is the real starting point and efficient cause for the development of spines, while the mode of their production has been through spontaneous variation and Natural Selection.[2]

A few remarks may now be added on the general question of adaptation in the vegetable kingdom. Reference has already been made to the numerous cases in which the special adaptations of flowers to insect-fertilisation can by no stretch of imagination be imputed to the direct action of insects, and the same thing is equally clear in many other directions. The whole group of insectivorous plants, for instance, exhibit strange and complex adaptations which have no

[2] Professor A. Kerner gives an admirable account of the various forms of spiny and prickly plants, which are exceedingly numerous in the Mediterranean district, and he adds: "In northern regions not exposed to summer drought, where grazing animals find in summer enough green fodder, this form of plant is almost entirely absent." ("The Natural History of Plants," English Translation, vol. i., p. 445.)

direct relation to the mere fact of insects crawling over them or settling upon them. So also are those varied adaptations by which, as Kerner has shown, injurious insects are prevented from reaching the flowers.

Even more unintelligible on this theory are modifications of fruits and seeds, by which some attract birds or mammals to eat them, while others are guarded against being eaten; some seeds have beautiful wings or plumes for wind dispersal, others have hooks or sticky hairs which cling to wool or feathers, while others again are scattered abroad by the sudden elastic bursting of the capsules. Take the comparatively simple case of nuts. Did they acquire their hard covering and brown protective tints and detachment from the tree as soon as ripe by the direct agency of birds, or monkeys, or squirrels? Of course, the question is absurd, since those eaten by these creatures could not transmit their special qualities; but those that, by the possession of any of these qualities, escaped being eaten, would transmit those qualities to the next generation.[3]

Any conceivable direct action of the environment can therefore have produced only a very small portion of the modifications and adaptations that actually exist. In by far the larger number of cases no such explanation is possible, and no other adequate explanation has been suggested except variation and Natural Selection. It is, of course, admitted that the action of the environment does produce definite changes in all organisms, more especially in plants, but there is no evidence that such changes are transmitted to the offspring of the individuals in which they have been produced.

On the other hand, there is direct evidence that many such changes are not transmitted, an example of which is the *Arabis anachoretica* with remarkable tissue-papery leaves, due to its growth in hollows of the rock, where neither sun nor rain reach it. Seeds of this plant when cultivated at Kew produced the common *Arabis alpina*. The same thing occurs with many plants, as every cultivator knows; but other forms with no greater peculiarities externally preserve their characters under cultivation, though exposed to the most varied conditions. As we thus know that *some* variations directly due to the environment are *not* transmitted, and also know that an immense number of spontaneous or congenital variations *are* transmitted, since by taking advantage of this fact almost all the improvement in our domestic animals and cultivated plants has been effected; and yet further, that no case has been found in which such spontaneous variations are wholly intransmissible,—the logical conclusion is that the two kinds of variation are distinct in their nature. This view of the subject is adopted by those botanists who are now endeavouring to determine the true nature of the

[3] Other cases of the want of relation between adaptations and their supposed cause are given in my article, "Are Individually Acquired Characters Inherited?" in the *Fortnightly Review* of May, 1893, pp. 664–9.

numerous alleged species, sub-species, and varieties of our native plants. They test the fixity of the characters which distinguish each form by cultivation. If these characters remain unchanged, and are transmitted by seed, the form is a permanent one and deserves to be recorded as a species or sub-species ; but if, as frequently occurs with forms which appear quite as distinct as those which are stable, the plant reverts on cultivation to some other form, it is evidently a modification due to some local conditions of the environment, and should be treated differently. Mr. Beeby has proposed to call the former "intrinsic," the latter "extrinsic" varieties, terms corresponding to Weismann's "germ variation" and "somatic variation," and these can in many cases only be distinguished from each other by the test of cultivation under different conditions. On this point Mr. Beeby remarks :—"The most transient states of plants due to the direct action of their environment are often far more distinct in appearance from their normal forms than are some varieties from their types ; but the first-named return at once to their normal state on being removed from their special surroundings, while the latter remain permanently distinct from their types even when grown under circumstances most disadvantageous to the continuation of the particular variation. That these two kinds of variation exist in plants is certain ; and the separation of them seems to be the very basis on which all investigations of the Phanerogamia must be made, if it is hoped that this branch of botany is to throw any further light on Evolution."[4]

In conclusion, I submit that the whole body of facts in relation to the direct action of the environment indicates that modifications thus produced in the individual are not transmitted to the offspring ; and that until it is demonstrated by experiment that they are so transmitted, theories of plant modification founded on that assumption are altogether worthless.

ALFRED R. WALLACE.

"On the Flora of Shetland." *Annals of Scottish Natural History.* January, 1892, p. 52.

II.

Hertwig's "Preformation or New Formation."[1]

Part II.—Criticism of Weismann's Theory of the Germ-plasm and Doctrine of Determinants.

ALTHOUGH Weismann has carried out his theory into the greatest detail, it is not with details so much as with the fundamental principles of it that Hertwig disagrees. It is with these principles, as they concern what is known about cells, that the chief difficulties seem to him to occur. No doubt, as Naegeli has said, there are many units of different orders above and below the cell, but the observation of the last thirty years has made it more and more certain that the cell is the most striking morphological and physiological unit of the body. Every theory of heredity must be tested by comparing it with facts observed about the cell. The cell, consisting of protoplasm and a nucleus, is an elementary organism, which, by itself or in combination with others, forms the basis of all organisms. It has an inconceivably complex structure, which, in its minuter parts, eludes our observation, and it is composed of many physical substances. Of these, albuminates, fats, carbohydrates, water, salts free or in solution, serve as material for the growth of the cell. Others form the living cell-body, in the narrow sense: by growth and division they can multiply and form the elementary unit of which the cell is a higher combination. In this category of intra-cellular units would be placed, if they existed, such theoretical structures as the gemmules of Darwin, the physiological units of Herbert Spencer, the bioplasts of Altmann, the pangenes of De Vries, the plasomes of Wiesner, the idioblasts of O. Hertwig, and the biophores of Weismann. The cell of each species has its specific organisation of a simpler or more complex structure, and contains a proportional number and variety of organised lower units. An unfailing organ of every cell is the nucleus, which is composed of elementary living units, the idioblasts, which differ chemically, morphologically, and physiologically from the units of the protoplasm, but may, perhaps, be able to turn into them. As a matter of observed fact, the

[1] Zeit- und Streitfragen der Biologie. By Professor Dr. Oscar Hertwig. Pamphlet I. Präeformation oder Epigenese? Grundzüge einer Entwicklungstheorie der Organismen. Pp. 144, with 4 illustrations in the text. Jena: Gustav Fischer, 1894. Price 3 marks.

nucleus is the bearer of the hereditary plasm, and may be inferred to be more stable than protoplasm and less subject to outer influences. It is the inherited plasm of the nucleus that stamps the specific character of the organism. The capacity of the cell to feed, grow, and divide is the mechanism by which the continuity of life is secured.

The cell plays a leading part in Weismann's theory. His germ-plasm is made up of primary units, the biophores, which are built up into determinants, a determinant existing for each independently varying cell or group of cells in the adult organism.

The arrangement of the biophores in the determinants, and of these in the ids or higher units, follow an elaborate historical architecture. This complicated germ-plasm lies in the nucleus, and nuclear division is the mechanism by which it passes from cell to cell. An individual life-history begins with a fertilised egg-cell containing paternal and maternal germ-plasm, which have come respectively in the nucleus of the ovum and in the head of the spermatozoon. In the actual course of ontogeny two things happen. By a series of nuclear divisions some of the germ-plasm is saved in an unaltered form, and ultimately gives rise to the sexual cells of the new individual. The structure of the rest of the germ-plasm in the course of another series of nuclear divisions gradually breaks down in such an orderly fashion, that the various determinants are marshalled to their proper places until ultimately the tissue cells of the various parts of the embryo and the adult, instead of containing a complicated plasm, contain only the determinants of their own order. Thus nerve-cells contain only the determinants of nerve-cells, muscle-cells of muscle-cells, and so forth. In certain special cases, which Weismann believes to be secondary adaptations, in addition to the specific determinants, cells may contain some other latent determinants, to provide for the regeneration of lost parts and so forth. But, with these exceptions, there is in every organism an impassable separation between the unaltered germ-plasm, which has been preserved to pass into the sex-cells, and the specialised simplified nuclear plasm of the various independently varying parts of the organism. Moreover, inasmuch as the unaltered germ-plasm of the sexual cells is continuous from generation to generation, while the part of it which breaks up into the nuclear matter of the various tissue constituents perishes with these at the death of the organism, Weismann distinguishes the one as the immortal germ-plasm, the other as the mortal somatic plasm.

Comparing this theory with actual knowledge of cells, Hertwig points out that it implies two quite different kinds of nuclear division. That kind of nuclear division by which the unaltered germ-plasm is handed on from cell to cell until it reaches the new sex-cells may be called " heirs-equal " division (erbgleiche Theilung), for in it the nuclear matter is distributed equally and unaltered between the two daughter-cells. That kind of nuclear division which occurs

while the various determinants are being marshalled to their places may be called "heirs-unequal" division (erbungleiche Theilung), for in each stage of it the daughter-cells receive different sets of determinants. Thus, if, as Weismann supposes, the first cleavage plane determines the longitudinal axis of the future body, the daughter-cell on the right would receive all the determinants for the right side of the body, the daughter-cell on the left all the determinants for the left side. This process of heirs-unequal division would be the characteristic method of nuclear division in all ontogenies, heirs-equal division occurring only in the few cases necessary to separate off the sexual cells. Later on in development, when the determinants have been completely sorted out, heirs-equal division would re-establish itself, for a determinate or independently variable part of the body generally would consist of a large number of cells resulting from the simple numerical multiplication of the cell or group of cells which, by heirs-unequal division, had received the complete set of determinants for the tissue in question.

Now Hertwig strikes at the root of all this process, and therefore at the root of the whole Weismannic theory of heredity, by saying that heirs-unequal division does not occur. Against it he brings up five groups of facts.

Groups of Facts against Heirs-Unequal Division.

I.—*Unicellular Organisms.*

Among single-celled organisms, we know only of heirs-equal division. It is the means by which the species are perpetuated. Were the division a case of heirs-unequal, then there would result, not daughter-organisms of the same species, but daughter-organisms of new species, and all our evidence is against this happening. But although the nucleus divides equally, it is not necessary that the organisms of the new generation be at once like their parent. In many protozoa an ontogeny is gone through. The results of division, for instance, in the fixed Acinetan *Podophrya gemmipara* are free-swimming ciliated forms, which, after a vagrant period, settle down into the fixed adult form. Similarly, among Gregarines quite complicated larval phases are gone through. None the less, in these and similar cases, the exact adult form finally appears. From such occurrences the inference is plain. Because cells are unlike we must not conclude that their nuclear plasm is unlike. To use Weismann's phraseology, some of their determinants may remain latent for a time, and while the complete adult form will appear ultimately, in the meantime the cell may present appearances very different.

II.—*Lower Metazoa and Metaphyta.*

In the case of such organisms as the filamentous Algæ the filament is, so far as can be judged, produced by heirs-equal division. Yet any of the resulting cells, similar to all appearance, may turn into sexual cells. If, with Weismann, it is assumed that the distinction between

somatic and sexual cells, once it has arisen, is inviolable, then we are led to strange conclusions. For instance, in two forms so nearly allied as *Volvox globator* and *Pandorina morum*, we should have to assume that *Pandorina* had no soma, for all its cells can reproduce: while in *Volvox*, where certain cells are specialised as sexual cells, there is a soma.

III.—*Reproduction and Regeneration.*

A multitude of cases show that very many cells and tissues contain the possibility of reproduction. Thus, a plant like *Funaria hygrometrica* may be chopped in tiny pieces, and each piece, placed in damp earth, will reproduce the whole plant. Similar occurrences are familiar in cases like the willow, the begonia, or in many coelenterates, worms, and tunicates. It is making a large drain on the imagination to suppose that in so many widely-scattered cases a condition diametrically opposed to what Weismann regards as the normal condition of complete separation between soma and germ-plasma has arisen under special conditions.

IV.—*Heteromorphosis.*

Loeb uses this term to denote the power of organisms, under the stimulus of outer conditions, to produce organs on parts of the organism where they do not occur normally, or the power to replace lost parts by parts unsimilar to them. Regeneration is the reproduction of like parts. Heteromorphosis is the reproduction of unlike parts.

If one cuts off part of the stem of almost any plant, on placing the stem in suitable soil, roots will grow out, although roots are not natural to that part of the stem. The prothallus of ferns produces the male and female organs on the lower side turned away from the light. If a prothallus be darkened on the upper surface, and illumined by reflected light on the lower surface, then the antheridia and archegonia will be produced on the upper surface. Galls are produced under the stimulus of the insect almost anywhere on the surface of a plant. Yet in most cases these galls, in a sense grown at random on the surface of a plant, when placed in damp earth will give rise to a young plant. In the hydroid *Tubularia mesembryanthemum*, when the polyp-heads are cut off, new heads arise. But if both head and root be cut off, and the upper end be inserted in the mud, then from the original upper end not head-polyps but root filaments will arise, while from the original lower end not root filaments but head-polyps will grow. In *Ciona intestinalis* round a slit cut into the body-wall a tubular process grew out, forming a new mouth, while round the base of this, a series of eye-spots, corresponding to the eye-spots round the real mouth, appeared. In all these cases, it is plain that there were present in parts affected the determinants, to use Weismann's term, not only of the normal parts, but also of parts which, under normal conditions, would never have appeared there; and these new parts growing in

the unwonted places bore the normal species-stamp as characteristically as similar parts grown in their normal places. It can hardly be supposed that the architecture of the germ-plasm contains special determinants to be ready for occurrences so casual, especially as these are called into existence by circumstances quite foreign to the normal environment of the organisms. On the other hand, the facts are consonant with Hertwig's belief that, as all division is heirs-equal division, all the species-characters that depend upon cells are latent in every cell.

The experiments of Driesch, Wilson, and Hertwig upon the early stages of developing ova show that heteromorphosis begins with the very earliest divisions of the egg. Thus Driesch, working upon echinoderm embryos, was able to flatten out the stage where there was a sphere of sixteen cells into a flat plate where all the cells were in the same plane. In such a plate, the nuclei of the cells occupied relative positions very different from the normal conditions. Yet from these Driesch obtained normal plutei larvæ. It was, in fact, as if the cells could be pushed about like billiard balls without destroying the future shape and characters of the embryo. Did each cell contain only the determinants that would correspond to the structures that would arise from it under its normal conditions, then change of its normal position would have arrested development. Each cell must, on the other hand, have contained the determinants for all the animal, and have allowed those to come into operation that were required by the new positions into which the cells were forced. Driesch, by separating the first two and the first four segmentation-spheres of an *Echinus* ovum, obtained two or four normal plutei, respectively one-half and a quarter of the normal size. Here again each sphere must have contained all the determinants for the whole organism. Heirs-equal division must have occurred. So, also, in the case of *Amphioxus*, Wilson obtained a normal, but proportionately diminished, embryo with complete nervous system from a separated sphere of a two- or four- or eight-celled stage.

Hertwig himself, some years ago, published the results of experiments he made upon the development of frogs' eggs under abnormal conditions. He showed that there could be no question of imperative divisions separating the germ-plasm into right and left halves, and so forth, but that the method of division was determined by pressures and relative gravities. Alteration of these made the ova divide into novel but symmetrical forms. Chabry obtained normal embryos in cases where some of the segmentation-spheres had been artificially destroyed.

These cases all show that in its possibilities each segmentation-sphere is identical: that as a result of heirs-equal division, each cell contains all the material necessary to cause the development of a complete embryo. Weismann would have to suppose that in all these cases, in addition to its half of the nuclear matter resulting

from heirs-unequal division, it had also a stock of unaltered germ-plasm ready to be called into activity by unwonted stimuli. But even this hypothesis would not account for cells distorted by compression responding with the production of unwonted symmetries.

V.—*Vegetative Affinities.*

It will now be clear that Hertwig considers that all cells, in addition to their apparent characters, possess, in addition, all the characters of the species.

For instance, histologically considered, the similar tissues in a group of animals such as the mammalia, are, in most cases, not to be distinguished. No doubt an expert could, in some cases, distinguish through the microscope the red blood corpuscles of certain mammalian groups from each other; but he could not do so in the case of species, while with most tissues—connective tissues, nervous tissues, muscular tissues—it would be only in rare and extreme cases that he could distinguish one mammal from another.

But, in addition to their visible structure, which bears an obvious relation to their function, Hertwig attributes to each cell hidden constitutional or specific characters. This is the case plainly enough with the sexual cells. From indistinguishable ova and spermatozoa, there arise animals with different specific, generic or even family characters. But the facts of translation and transfusion for animals and grafting for plants, show that there is a parallel between tissue-cells and sex-cells so close as to suggest that tissue-cells are as much specific as sex-cells.

In plants, the operation of grafting is very easy to bring about; yet it is found that the approximated shoots will not always unite; if the cases where grafting succeeds and fails be considered, it is found that they are parallel to success or failure in sexual crossing. Near allies will unite, but failure follows the attempt to bring together shoots from different species. There are, it is true, some notable exceptions—cases where plants, considered closely allied, will not unite, and cases where plants, regarded as specifically separate, will unite. Grafting follows, not the apparent outward tokens of resemblance, but the vegetative affinities of the cells. It is their latent constitutional characters, and not their patent tissue characters, that are at work. It is upon the experiments of Voechting that most of these conclusions depend. He found in the case of the pear and the apple, which belong to the same genus, that grafts would not succeed, and in their case sexual crossing will not occur; but the pear and the quince do graft, although these belong to different genera. Hertwig thinks it probable that sexual affinity and vegetative affinity depend upon the same ground characters of the cell.

Voechting calls the grafting of twig and stem *harmonic* when a full individuality is reached, *disharmonic* when this is not the case. When the disharmony is complete the two either poison each other

or develop separately. When, however, an intermediate condition exists, growth together may go on for a time. Then, after a time, the twig grows rootlets for itself, which penetrate the stem of the other, and, trying to achieve its own individuality, becomes a parasite instead of a part of the other plant. Sometimes plants, between which disharmony exists, may be brought together by a third. Thus plant A, which will not grow upon B, may have a third plant C, such that when C is grafted upon the stem of A, then B can be grafted upon C.

Resembling the conditions of vegetative affinity are the conditions of sexual union. Thus, infertility is graded from being complete to partial. Sometimes only the first cleavage occurs ; sometimes a gastrula and so forth, but death ensues from no outward cause.

Animal parallels, such as transplantation, are rarer. Trembley succeeded in cutting two specimens of *Hydra fusca* across and getting, in one case, the cut halves of the different animals to unite. But he could not graft upon *Hydra* part of another hydroid.

Ollier succeeded, after removing a part of the periosteum from one spot in an animal, in making it grow and become bony in another part of the same animal. But it was an evanescent success, for the new growth being a foreign object in its novel place, speedily became absorbed. In the case of transplanting from the dog to other animals like cat, rabbit, etc., either the new growth got absorbed, or it festered, or became enclosed in a cyst.

Paul Bert transplanted part of the tail of a young rat to another part of the body. There it grew, all the tissues but the nervous developing well. Transplantation of the tail to other animals was more difficult. Usually it festered, and nearly killed the animal. Sometimes it was resorbed ; only in cases of very nearly-allied animals did it grow.

A. Schmitt recently found that pieces of skin transplanted to animals of another species invariably festered out, or were quietly resorbed.

In the case of transfusion of blood, failure always was the result of experiments between animals of different species. In large doses it was fatal ; in small, harmful ; and always, in a very few minutes, the transfused blood corpuscles began to degenerate, their hæmoglobin appearing in the plasma. Landois and others conclude that the results of transfusion correspond with the anatomical affinities of the animals experimented upon.

Hertwig concludes that the cells, in addition to their tissue characters, have species characters, and that as one can speak of the sexual affinities of the sex-cells, so one can of the vegetative affinities of the tissue-cells.

Before proceeding to give an account of Hertwig's further criticism of Weismann's theory of heredity, it is worth while to sum up the part of the argument already treated. Upon Weismann's

theory, the part of the germ-plasm required to direct an individual ontogeny gets broken up and distributed to the various cells of the embryo by a series of heirs-unequal divisions. By this means the cells of the soma come to contain only the determinants which can produce cells of their own order. The exceptions are not numerous. There is, first, the great exception by which unaltered germ-plasm is passed along the germ-tracks to the place of origin of the sexual cells. Next there are a few special cases of adaptation to the needs of regeneration and bud formation in which a certain amount of reserve nuclear matter is retained. Against this Hertwig asserts that all cell-division is by heirs-equal division, and, therefore, every cell, in addition to its patent characters, retains the power of producing on emergency all the species-characters. He goes on such facts as these. In single-celled organisms all cell-division obviously is heirs-equal division. In the lower animals and plants, almost any cell, when occasion demands, may become a sexual cell. Regeneration of lost parts, and still more heteromorphosis, show that each cell is not limited in its capacity to its predestined sphere of work, but can reproduce totally distinct parts of the organism, and can reproduce them in such a way that the specific characters are maintained. The results of grafting, transplantation of tissue, and transfusion of blood, show that cells beside their patent tissue-characters contain latent species-characters, and that, generally speaking, the resemblance between sexual affinities and vegetative affinities shows that tissue cells are as equally specific as sexual cells.

Considerations Against the Doctrine of Determinants.

With his doctrine of heirs-unequal division, Weismann has united his theory of determinants in the germ-plasm of the fertilised egg-cell. Each independently varying part of the adult is represented by biophores united into a determinant. The various determinants are arranged in the plasm or are endowed with such powers that they come liberated at the proper time to enter into the cells to form the determinate. Hertwig's criticism of this, and his arguments against it, are strongly epigenetic. In the modern, as in the older, doctrines of preformation, he sees the same confusion between sequence and causation. If the ancestor of the whole chain of animal-life were an amœba, it would be an empty and meaningless phrase to say that the amœba contained in any real sense the material germ of all the subsequent animals that arise from it. Similarly, although all the cells of the body have their origin from the egg-cell, it is no necessary consequence that the material germs of all subsequent tissues and cells are contained in the egg-cell. The egg and the adult do not form a closed chain of forces. Many other conditions must be present.

Every organic development depends, for instance, upon the absorption and metamorphosis of nutritive materials. The cells grow

at the expense of material taken from the outside, and what in one stage is raw material becomes in the next the starting point of the next cell-generation. Thus food-yolk enters the cells, and, though the difference is only quantitative, produces a qualitative change in the embryo. If one thinks of the number of stages between the egg and the adult, and that each stage has had a large absorption and change of foreign material, it gets plain that it is an error to put into the egg all the incipia of adult structures, as many of these come into existence only at later stages of development.

A second error in the theory of determinants is to attribute to cells—as, for instance, to the ovum and spermatozoon—properties which are not cell properties, but those of combinations of cells. Of the characters of animals and plants, many are due to the co-operation of the whole organism, others to single organs or to groups of cells. If these are to have physical carriers in the germ, are the carriers biophores or determinants? Hertwig can admit for a cell only characters peculiar to cells. A sexual cell can contain physical incipia for chondrin, ossein, pigment, chlorophyll, nerve fibrillæ, but not for the formation of a definite spinal ganglion, a hair, or the biceps muscle. The incipia of these must be groups of cells.

The egg is an organism which, by feeding, growth, and division, can split up into exactly similar organisms, and it is first through the inter-relations of these new organisms that in each stage of development the combined organism makes its stages of growth. Criticising the pangenes of De Vries, Weismann says that there cannot be zebra-stripe pangenes, and so forth. There could be black and white pangenes, for these are cell properties; but the zebra stripe and many similar characters depend not on cells but on the arrangement of cells. Hertwig accuses him of falling into precisely the same error in his determinants. There cannot be in a cell the determinants of organs or structures which depend not on cell characters but upon the relations between cells.

Thus the human state depends upon the co-operation of many classes of men. Supposing, for the argument, all men to have come from a single pair, this pair, according to Weismann, would contain the determinants of the whole state. But the co-operation of individuals into groups, and of groups into the state, is something new which could not exist while there was only a single pair. Of course the subsequent results are based upon the nature of men, but in no mechanical fashion are they contained in the single pair. So far as causal relations exist between the egg and the organism, the parallel holds exactly. There can be no material particles present in the egg as the incipia of characters due to the co-operation of cells. As Naegeli says, to understand heredity we do not require a separate individual symbol for each difference caused by space, time, and property, but we require a substance which, by the linking of its limited elements, can represent every possible combination of differ-

ences, and through permutation can pass into other combinations. As Hertwig puts it, the hereditary material contained in the egg and spermatozoon can be made up only of those material particles which are the bearers of cell properties. Every composite organism can inherit its characters only in the form of cell characters. The numerous indefinitely variable properties of animals and plants which are expressed in the different form, structure, and function of their different organs and tissues, and in the different relations between them, are functions of cell complexes. They depend on the interaction of many cells, and cannot have as their hereditary bearers material particles in a cell. They are new structures which come into existence only through the combination of cells with different individual properties.

Take as an example of the impossibility of determinants, the early stages of the frog's development.

In cleavage the nucleus plays the chief part, but to start this there is needed not a special determinant, but the co-operation of all the functions of the cell through assimilation of nutriment from the yolk. The chromatin fibrils, which we can represent as independently growing and dividing elements, have to be doubled in number. The centrosome also must be independently doubled. All these processes are the result of chemical and other changes depending on the powers of the cell as an elementary organism, and extended to the chromosomes as units of a lower order. The division of the nucleus into two, four, eight, and so forth, gives the stimulus to the yolk-mass to divide, and this exercises a strong influence upon the arrangement of the cells and the direction of the planes of cleavage. But these are not due to special determinants, for many of them are due to the special properties of the yolk. Thus Hertwig himself has shown that the direction of the planes of cleavage depends upon the relations of the specific gravities of the various parts of the cell, and that the unequal size of the cells and the unequal rate at which they divide depend upon the separation of the protoplasm into parts richer and poorer in yolk granules. No doubt the first three cleavage planes often coincide with the future axes of the animal, and this is laid hold of by Roux and Weismann as proof of their view that these planes separate cells containing material for the right and left halves, and so forth. But these also Hertwig thinks he can show to result not from specific determinants, but really to depend upon the shape of the egg and the yolk distribution. Thus the shape of the egg itself in many cases determines the necessary shape of the adult. In the case of eggs, those with polar arrangement of yolk give rise to polar segmentations, those with equal distribution of yolk give rise to holoblastic division. In meroblastic eggs the richer or slighter assembling of yolk, and therefore of specific gravity, is the determinant of the special kind of embryo. In very many cases where the cleavages give the future axes, these are determined not by the

nucleus, but by the distribution of the yolk and by the shape of the embryo.

The formation of the blastula depends on several factors.

1. The cleavage into four, eight, and sixteen causes a loosening of the cells and the appearance of intercellular spaces.

2. As the cells multiply rapidly they naturally come to be pressed into an epithelial structure.

3. As the outer cells become pressed into an epithelium, the segregation of fluid and arching of the outside cause a central cavity to appear and grow larger.

Where is the room for special determinants? The division is an elementary property of all cells; the appearance of intercellular spaces is a result of cellular and external forces as the nucleus forms an attraction centre round which the yolk tends to arrange itself equally.

Gastrula formation is a result of the co-operation of all the cells, of the inequality in curvature, and of a series of external circumstances. To attribute this to determinants is to turn things round. It is not because certain cells contain certain elements that they become epithelial, digestive and so forth; but it is because, by mechanical and other reasons they come to lie in special spaces, that the corresponding side of their character gets developed.

Hertwig's leading objections to Weismannism are then, first, that he thinks the evidence is in favour of cell-division being heirs-equal division, and that, in consequence, the germ-plasm is not separate from the soma, but all the cells in an organism contain the cell-characters necessary to produce a complete organism. Next, a large number of the characters of organisms depend upon combinations of cells, and so cannot have determinants in a single cell. His view is epigenetic in a double way. Each cell contains all the cell-characters of the organism: the special characters which become active are educed by its environment. Characters which depend upon the co-operation of cells can come into existence only by the combination of cell-characters after these have been found. In the next number of NATURAL SCIENCE I hope to give an account of the concluding portion of Hertwig's pamphlet.

P. CHALMERS MITCHELL.

III.

The Parasites of Malarial Fevers.[1]

NO one to whose lot it may fall to have to travel alone in tropical countries can afford to neglect the study of the various diseases there prevalent. This is necessary not only for self-protection and self-cure, but for sake of the influence that may be thus acquired over the natives by a rapid cure or sudden soothing of pain. Of all the diseases which it is necessary for such a one to consider, there are none more important than those of that puzzling series grouped together as malaria. For long it was thought that many ailments included under this name had no connection, for their causes were quite unknown. During the last fifteen years, however, great advances in the knowledge of the subject have been made. England has taken but a very small share in this, as the disease is not one often met with in ordinary British practice. It is true that in the lower parts of the eastern counties there is a good deal of a malarial ague. A "West Coast" captain recently remarked that the malaria on parts of the Essex coast is as bad as it is anywhere in Africa; but this need not be taken seriously, and only serves as a further illustration of the fact that a West Coast captain will say anything. In the tropics, on the other hand, the disease is one of the most important, but the conditions for its study are not as favourable as might be wished. In Italy, however, both the disease and skilled investigators occur together. There malaria is so prevalent and assumes such a pernicious type, that it has been a matter of study since the earliest days of medical science. Thus, even in the time of Hippocrates, so closely had it been watched that the fevers were then divided into two classes.

The literature of recent work upon the subject is very scattered, but a general sketch of the whole subject has been given in two monographs, of which a translation has been issued by the New Sydenham Society as the 150th volume in its series. These treat the subject from somewhat different points of view. The first is rather

[1] Two Monographs on Malaria and the Parasites of Malarial Fevers. New Sydenham Society. 8vo. Pp. xxvi. and 428, six plates, three charts. London, 1894. I.—"On Summer-Autumn Malarial Fevers." By E. Marchiafava and A. Bignami. Translated from the Italian by J. Harry Thompson. II.—"The Malarial Parasites." A description based upon observations made by the author and by other observers. By Julius Mannaberg. Translated by R. W. Felkin.

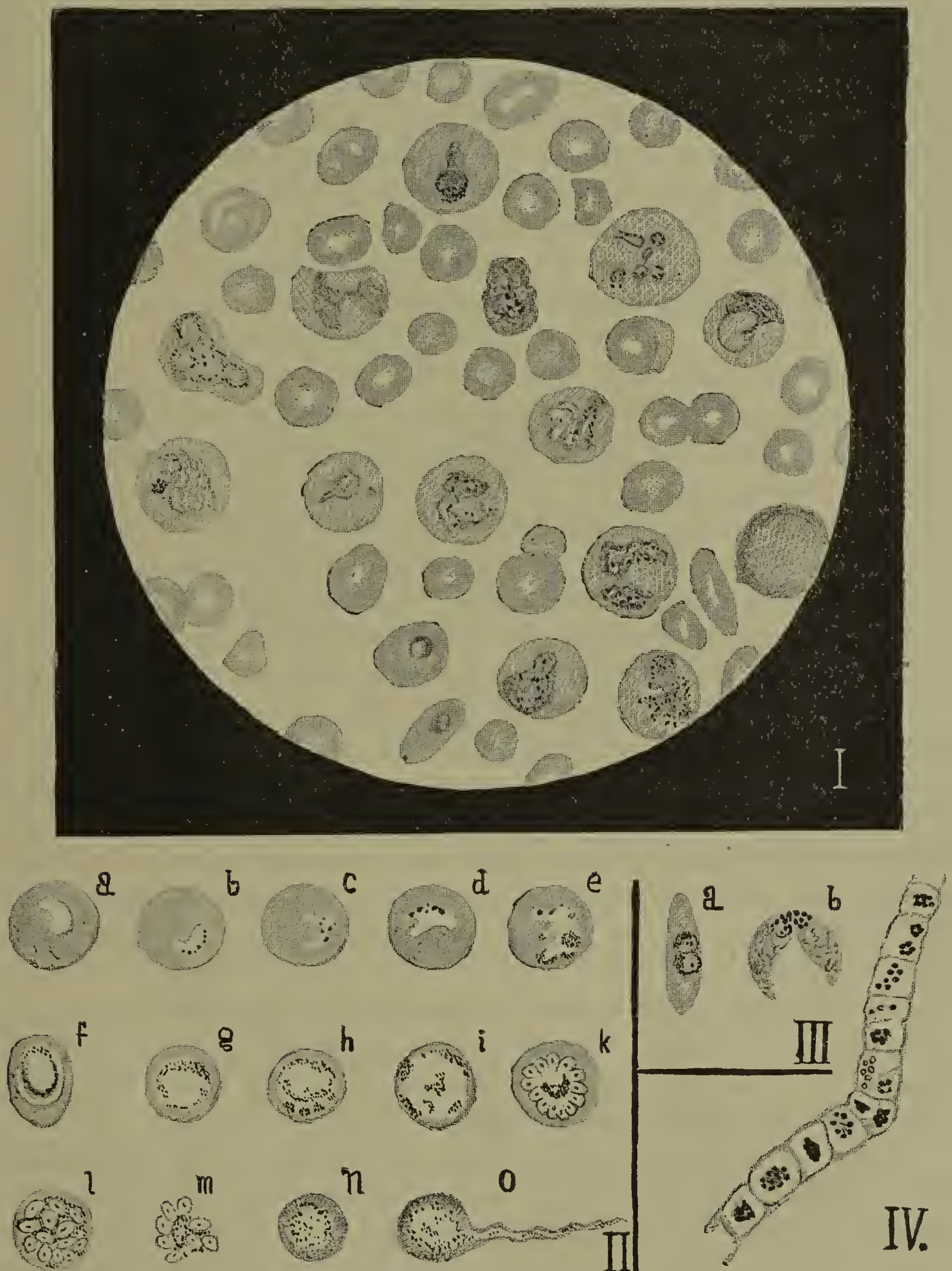
medical in its treatment; it is a study of the worse group of the two groups of fevers of the Roman Campagna: it is entitled "On Summer-Autumn Malarial Fevers," and is by Professor E. Marchiafava and Dr. A Bignami. The second, mainly biological, is on "The Malarial Parasites," and is the work of Dr. Mannaberg, of Vienna. The former is admirably translated by Dr. Thompson, of Washington, who has added an interesting preface of his own: not so much can be said for the translation of the second part of the volume, where misprints, errors in translation, and unintelligible sentences detract from the value of the work.

The study of the subject has an important bearing on some biological questions. It may be of interest, therefore, to attempt briefly, in non-medical terminology, to summarise some of the results for the benefit of general students of biology.

The dawn of modern work on the subject began with Heinrich Meckel's (1) discovery in 1847 of pigment in the blood of a dead patient who had suffered from malaria. The significance of this, however, was not seen till, in 1879, Marchiafava and Celli began their brilliant course of investigations into malaria, by proving that this black pigment or "melanin" was formed within the red blood corpuscle from the hæmoglobin as a product of disease. The next year the great modern impetus to the study was given by the announcement by A. Laveran (2) that he had seen a flagellate organism in the blood of a patient suffering from malaria, which he maintained was the cause of the disease. From that time the study of the subject has made great strides. Laveran appears to have rather jumped at conclusions, and these were therefore not accepted at the time by many of the most careful investigators; thus Marchiafava and Celli dismissed these parasites as merely the products of the decomposition of the blood corpuscles. Nevertheless, the truth of Laveran's main conclusion has been verified, and it is now undoubted that the various types of malarial fevers are due to organisms in the blood.

As in our English ague, the first and most striking feature in these malarial diseases is their recurrence at certain regular intervals. In the old days this could not be properly explained, but it is now known to be due to the fact that the malarial parasites pass through a regular life-history. If the blood of a malarial patient be examined with suitable precautions, with the best type of modern instruments, it will be found to present somewhat the appearance shown in Fig. I., which is magnified a thousand diameters. There it will be seen that many of the blood corpuscles contain foreign parasitic bodies: these are either small and rounded, and occupy only about a tenth of the corpuscle, as shown in one at the lower margin of the figure. These are immature parasites, and their development may be illustrated by the series shown in Fig. II. Here the round discs represent the blood corpuscles: in *a* we see the parasite in its most primitive condition, living inside the corpuscle. It feeds upon the material of its host, and secretes the

red colouring matter or hæmoglobin which it takes in, as small scattered grains of a black pigment or melanin: further stages show the growth of the parasite and increase in the amount of pigment, until in Fig. *i* the whole corpuscle is occupied by the parasite. Nutrition is thus stopped and spore formation commences. In *l* this process is com-



Parasites of Malarial Fever.
(For explanation, see end of Dr. Gregory's Paper.)

plete and the next figure shows the free spores. The last two figures of the series show a less normal development that sometimes takes place: here the whole parasite is set free by the rupture of the blood corpuscle. The particular species illustrated in Fig. II. runs its life-cycle in three days, and therefore produces the so-called quartan

fever in which, after one paroxysm, the next occurs on the fourth day. If the blood of a patient be examined on the first day in a case where the fever has a single origin, then only such forms as those represented in Figs. *a—c* will be seen; on the second day will be found the forms with the larger hæmatozoa—as the parasites may be called—such as those shown in *h* and *i*. During this period the patient does not suffer fever: if the disease be of the intermittent type the temperature will be normal; if remittent, it will be a little above this point. Then, if the blood be watched, the spore formation (Fig. *k*) is seen to commence, and the temperature of the patient rises, probably by the secretion of some poisonous product into the blood. The paroxysm of the fever, during which the temperature may rise to 106° without serious danger, is always approximately synchronous with the setting free of a fresh generation of spores.

Numerous deviations from this occur: thus some fevers are quotidian and the hæmatozoa complete their life-cycle in one day, and others are tertian with a 48 hours' cycle. Moreover, in any type, many of the hæmatozoa do not complete their normal development. The most important series of irregularities, however, is due to the presence in the blood at the same time of more than one generation, or more than one species of parasite. This is seen in Fig. I. Thus in the lower part of the figure is a corpuscle with a hæmatozoon in the earliest stage, while scattered over the field are several fully developed and two which are undergoing spore formation. Owing to the presence of this latter series the patient is either in or very near to a paroxysm of fever. Twenty-four hours later the younger generation will have reached the spore-forming stage, and thus another paroxysm of fever will be brought about. Therefore, though the life-cycle of this parasite takes two days for its completion, the patient will have a daily attack of fever, as the two generations alternately reach the spore-forming stage.

Considerable difficulty has thus been introduced into the study of these malarial diseases, owing to the complications caused by the simultaneous presence of two or more generations of the same species or even of altogether different species.

This raises the question as to whether the various hæmatozoa are definite species or only forms of one or few species modified by different conditions. This has given rise to great difference of opinion. Laveran and his school maintain that there is but one species and that all the varying forms are due only to polymorphism; thus Laveran sums it up in his great monograph (3), "Du paludisme et de son hématozoaire," issued in 1891, that "Le parasite est unique, mais son évolution est variable." The Italian school, however, and Dr. Mannaberg agrees with them, maintain the plurality of the species; Grassi and Feletti defined two genera, *Laverania* and *Hæmamœba*, and numerous species, and these are now generally

accepted. Mannaberg gives an interesting summary of the discussion and of the observations and experiments whereby it has been proved that each fever is the result of its own special parasite, and that by no known means can one species be changed into another.

Now that the life-history of these parasites has been worked out their true zoological position becomes a matter of interest. Laveran at first assigned them to the vegetable kingdom under the name of *Oscillaria malariæ*; but this view was promptly abandoned by its author, and he renamed the species *Hæmatozoon malariæ*. Numerous other names have been suggested, such as *Hæmatophyllum*, *Hæmatomonas*, *Cystosporon*, *Hæmogregarina*, *Hæmoproteus*, *Polymitus*, etc., but the name most generally accepted is *Plasmodium*, by far the most inappropriate. Medical men usually refer to the parasites as "malarial plasmodia." It need hardly be pointed out to biologists how utterly inappropriate this name is; a plasmodium is an irregular mass of protoplasm *containing many nuclei*. The figures, reproduced here, show that these bodies have but a single nucleus. As Laveran's name, *Hæmatozoon*, was not defined with sufficient precision to allow of its adoption in zoology as a generic term, Mannaberg wisely proposes to use it as a general name for the parasites.

In regard to the class to which they must be referred, numerous opinions have been held: of these several merit notice. Metschnikoff assigned them to the Sporozoa, and placed them in the same group as the genus *Coccidium*. Antolisei, however, referred them to the Gymnomyxa, while Grassi and Feletti considered them to belong to the subclass *Amœbeæ* of the class Rhizopoda. Kruse, again, referred them to the Sporozoa, and to the class of Gregarines. Danilewsky (4) finally, in agreement with the most generally received opinion, placed them in the Sporozoa, but established for them a new class, the Hæmosporidia. Mannaberg, after a careful discussion of the subject and comparison of the characters of the parasites with those of the allied forms of Protozoa, agrees with Danilewsky. He divides the Sporozoa into five subclasses, the Coccidia, Gregarina, Myxosporidia, Sarcosporidia, and the new subclass of Hæmatosporidia.

The results of these researches are not only of interest biologically but of great practical value, for with a knowledge of the cause, it is possible to apply remedies with greater certainty than of old. One of the most interesting sections in each of the two monographs is that dealing with the action of quinine. This has long been known as the most efficaceous of all medicines, but its power has been assigned to very different methods of action, as, for example, to a stimulation of the nervous system. Binz (5), however, in 1867, laid the foundation of a sounder view by showing that, if Infusoria be subjected to the action of quinine, they are killed, and, further, that this is due to asphyxiation: the alkaloid destroys the power of the Infusoria to take up oxygen. The quinine acts on the Hæmatozoa

in the same way; it therefore acts only when the parasites are passing through the stages of nutrition and development. When all the hæmoglobin of the corpuscle which acts as the host has been converted into melanin, the parasite ceases to grow, but divides into spores for reproductive purposes, and quinine becomes powerless. It is therefore necessary to take quinine in the intervals between attacks of fever, and useless to do so when the pyrexia is at its worst. This was long ago determined empirically by tropical practitioners, who also understand that quinine really does as much harm as good if taken at the wrong time. The reason for this also is now explained, for Binz has also shown that quinine has an unfavourable action on the leucocytes, or white corpuscles, which are thus checked instead of being aided in their struggle with the disease. Thus an unintelligent use of this reagent may be attended with very unfavourable results. This is likely to happen as many of these malarial parasites are not checked by quinine: this is the case with those which cause malarial fever in birds, and with some forms of human parasites. For instance, those shown in Fig. III., and known as the "crescentic bodies of Laveran," are quite uninfluenced by this reagent. If microscopic examination be neglected, and quinine administered in strong doses under the idea that the fever is produced by the species illustrated in Fig. I., serious injury may result.

The researches embodied in these two monographs are of the highest interest, both to the biologist, from the detailed investigation of the life-history of these low Sporozoa, and to the doctor, from their guidance in the proper treatment of a very troublesome and obscure group of diseases.

EXPLANATION OF FIGURES.

I.—Blood infested by malarial parasites (after Mannaberg), magnified 1,000 diameters. It shows (1) normal red blood corpuscles (the round discs with white centres); (2) a normal white corpuscle or leucocyte (the large dark circular mass touching the lower right hand border of the figure); (3) red corpuscles infested by the parasite in its youngest stage (two shown one above the other, in the middle line, just above the lower margin of the figure); (4) red corpuscles almost fully developed (the large discs containing shaded bodies); (5) parasites in which spore formation has commenced (one occurs in the upper part of the figure, a little to the right of the middle line; it is a dark body, roughly oval in form.)

The hæmatozoa belong to a tertian form. Thus, as spore formation is just beginning (as shown by the large size of most of the older generation of parasites, and that in two spore formation is fairly advanced), the patient is approaching an immediate paroxysm of fever. As a second generation is also present this will cause another attack of fever next day, so the patient is suffering from a "double tertian fever." Quinine if taken now would kill the parasites of the second generation, have no influence on the older generation, and check the efforts of the white corpuscles and of the phagocytic cells in the spleen to eradicate the disease.

II.—*a–o*, the life-cycle of the hæmatozoa of a quartan fever. (After Marchiafava and Bignami). In *a–l* the parasites are shown in red blood corpuscles. In *a* the parasite is in its simplest unpigmented condition; *b–i* show the gradual

growth of the parasite and increase in the amount of pigment; *k* and *l*, spore formation; *m*, free spores; *n* and *o*, free pigmented degenerate forms.

III.—The "Crescentic bodies of Laveran." (After Mannaberg). Magnified 1,000 diameters. In *b* transverse segmentation is shown.

IV.—Capillary vessel from the brain of a patient dead from malaria comatosa; the capillary is choked by spore-forming bodies.

REFERENCES.

1. **Meckel, Heinrich.**—"Ueber schwarzes Pigment in der Milz und dem Blute einer Geisteskranken." *Allgem. Zeit. f. Psychiatrie.* Bd. iv., 1847, pp. 198–226.

2. **Laveran, Alphonse.**—"Note sur un nouveau parasite trouvé dans le sang de plusieurs malades atteints de fièvre palustre." *Mém Acad. de Méd.* Paris, Séance 23, Nov., 1880.

————"De la nature parasitaire des accidents de l'impaludisme." *Compt. Rend.*, t. xciii., 1881, pp. 627–630.

————"Traité des fièvres palustres avec la description des microbes du paludisme." Paris, 1884, pp. xiv. and 548.

3. ————"Du Paludisme et de son Hématozoaire." Paris, 1891.

4. **Danilewsky.**—"Sur les microbes de l'infection malarique aigué et chronique chez les oiseaux et chez l'homme." *Ann. Inst. Pasteur*, 1890, p. 753.

5. **Binz, C.**—"Uber die Einwirkung des Chinin auf Protoplasma-Bewegungen." *Arch. f. mikrosk. Anat.*, Bd. iii., 1867, pp. 383–389.

————"Chinin als Hemmniss eines Oxydationsvorganges." *Centralb. f. med. Wissensch. Berlin*, Bd. vi., 1868, p. 481.

————"Resultate neueren Untersuchungen über das Wesen der Chininwirkung." *Verh. naturhist. Ver. preuss. Rheinl. und Westph.*, Bd. xxv., 1868, pp. 13–14.

————"Experimentale Untersuchungen über das Wesen der Chininwirkung." Berlin, 1868.

————"Protoplasmic Movement and Quinine." *Quart. Journ. Microsc. Sci.*, new ser., vol. xxiv., 1884, pp. 682–684.

————"Ueber Chinin und die Malaria-Amöbe." *Berl. klin. Wochenschrift*, 1891, no. 43.

J. W. GREGORY.

IV.

A British Palm.

THE mention of Palms carries us, in imagination, to a warmer climate than our own, as we figure the tall, stately, unbranched stem crowned by a few large spreading leaves. But all are not lofty; in some cases the trunk is very short, or there is only a stout underground stem terminating at the surface of the soil in a leafy crown; while a *Thrinax* recently discovered at Anguilla, in the West Indies, generally reaches only about a foot in height—a specimen 2 ft. 8 in. being described as quite tall—and a dwarf species of *Phœnix*, the genus to which the date-palm belongs, discovered shortly before in the East Indies, was likewise only about 25–30 in., forming, with its slender stem, a very attractive little plant.

Others are not even self-supporting, but climb or scramble over surrounding vegetation. Such are many of the tribe Calameæ, which includes the Rattans or Cane-palms (*Calamus*), with a very long slender stem, stated by Blume, in his "Rumphia" (vol. ii., p. 158), on the authority of Rumphius himself, to reach a length of 1,200 to 1,800 feet. This statement has not, however, been verified, though 300 feet is said to be a common length in Ceylon and the Malay archipelago.

The leaves of the scramblers often terminate in long appendages or flagellæ, armed with stout recurved hooks, which may be extremely formidable and dangerous, though of great service to the plant as holdfasts. In one of these, by the way, *Korthalsia scaphigera*, from the Malay peninsula, the stipules of the leaf are united and swollen, forming an oval, hollow, smooth-walled chamber, of which ants take possession as a home.

According to the "Genera Plantarum" of Bentham and Hooker, there are about eleven thousand species, many imperfectly known; while many more, without doubt, remain to be discovered in Africa, Central America, Madagascar, and the islands of the Pacific Ocean. The family is essentially a tropical one, the greater number being American, a few Asiatic and Australian, and very few African. In America, a few small genera are peculiar to the Southern United States, such as *Serenæa*, dedicated to the late Sereno Watson by Sir J. Hooker, and found in Florida and Carolina; *Washingtonia*, inhabitating South California and Arizona; and

Erythea, peculiar to the former. In South America, *Jubæa*, a Chilian genus, reaches as far south as the 37th parallel of latitude. The only species, *Jubæa spectabilis*, is popularly known as the Chili Coco-nut, and its fruits, looking like miniature coco-nuts an inch and a quarter long, are sometimes sold in this country. In the Western Hemisphere, the southern limit is 44° south latitude in New Zealand, the extreme form being a small endemic palm, classed originally as an *Areca* (one species of which, *A. catechu*, yields the well-known Betel-nut) but now separated, together with a closely-allied plant from Norfolk Island, as a distinct genus, *Rhopalostylis*. North of the Equator the monotypic (*i.e.*, containing only one species) genus *Nannorhops* lives on the mountains of India, beyond the Indus, and extends through Afghanistan and Beluchistan to S.E. Persia. The only native of Europe is a species of *Chamærops*, a small genus of dwarf palms confined to the Mediterranean region. *Chamærops humilis* is found in the south of Spain and Portugal and on the Mediterranean seaboard, reaching its northern limit at Nice at a latitude of 43° 44′. It is generally quite a dwarf, only three or four feet in height, and sends up numerous suckers in dense tufts. It may, however, form a trunk twenty or thirty feet high. Nyman, in his "Conspectus Floræ Europæ," speaks of it as the lowly and degenerate representative of a noble family afforded by the European flora, but reminds us that in the extreme south a loftier palm has found a home, viz., *Phœnix dactylifera*, the date palm. Beyond 38° or 39° latitude the date becomes merely an ornamental tree, as its fruit no longer matures, remaining more or less acid to the taste. It is cultivated in great quantity over the whole of North Africa, and more sparingly in Western Asia and Southern Europe. Like wheat, the sugar-cane, and many of our ancient useful plants, it is now never found in a wild state, and its original home is unknown.

Of course, pampered individuals in favoured spots will thrive in still more northerly latitudes. The Chusan palm (*Trachycarpus excelsa*) is sufficiently hardy to stand the winter in the north of France and even in the Isle of Wight. It has, however, a protection from the cold in the form of a coarse hair resulting from the decomposed bases of the leaf-stalks. The ordinary ratepayer may become acquainted with the family in the London parks during the summer months, where, by the way, I recently heard one described as "the kind of tree of which you see pictures with missionaries sitting under." It was labelled, incorrectly, *Seaforthia Cunninghami* in small letters and low down. "An Australian Palm" in larger print would have conveyed more information to the author of the remark.

The one hundred and thirty-two recognised genera of palms are somewhat restricted in their distribution. The *Raphia*, indeed, is a native of both Old and New Worlds, having several species in tropical Africa and Madagascar, and one in America, from the mouth

of the Amazon as far north as Nicaragua. Several of the tropical Asiatic genera, like *Areca*, run down through New Guinea into Australia. Others are extremely local. Lord Howe's Island has, for instance, two peculiar genera. *Juania* is found only on the small island of Juan Fernandez, while the small group of the Seychelles, in the Indian Ocean, boasts no less than five endemic genera. One of these, *Lodoicea*, bears the fruits known as the double coco-nut, or *coco de mer*. The great woody nuts were found floating in the sea long before the discovery of the tree which produced them or the islands on which it grew, and many absurd stories of their origin were in vogue among the earlier navigators.

Of somewhat restricted distribution, as well as of doubtful relationship, is the *Nipa* palm. Only one species is known, *Nipa fruticans*, which frequents the brackish estuaries and littoral marshes of tropical Asia from Ceylon to the mouth of the Ganges, through the Malayan Archipelago to the Philippine Islands, New Guinea, and tropical Australia. It is of dwarf habit, having a long, thick horizontal root-stock which sends roots into the ground and bears, at the apex, a number of long deeply-cut pinnate leaves. The male and female flowers are borne separately on a spadix or long fleshy stalk springing from among the leaves. The male are thickly crowded on catkin-like branches, the female borne in a rounded head terminating the spadix. The head of ripe fruits is as large or larger than that of a man. The individual fruits vary somewhat in size and shape, according to their position in the head, but are all more or less inversely egg-shaped, the upper-third projecting and rounded with generally an apical umbo, the lower portion pressed on all sides by neighbouring fruits and more or less angular, showing four to six obtuse angles. The coat is of a deep brown colour and polished, while the substance is thick and fleshy but densely fibrous. It contains a single seed with a small basal embryo and a quantity of cartilaginous albumen, which, however, is edible before the seed is quite ripe. The seed is said to germinate within the fruit, which remains on the spadix for several years, by which time the embryo has so far advanced in growth as to be unaffected by salt water. I think, however, that this statement requires confirmation. The fruit finally drops into the brackish water or mud and the seedling may grow where it falls, or float some distance. Great numbers of the fruits are often found floating in the river estuaries. Sir Joseph Hooker, in his delightful "Himalayan Journals," refers to the frequency with which they were tossed up by the paddles of his steamer in the Sunderbunds.

In the "Philosophical Transactions" of the Royal Society for 1757, Dr. James Parsons gave an account of some fossil fruits and other bodies found in the London Clay, in the island of Sheppey, by Edward Jacob, a surgeon of Faversham, who also subsequently published an account of them as an appendix to his "Plantæ Favershamienses"

in 1777. Of two of the fruits, figs. 1 and 3 on plate xv. of the Royal Society paper, Parsons says they seem to be figs petrified when hard and green. In 1785, James Douglas, in a work entitled "A Dissertation on the Antiquity of the Earth," refers to the Sheppey fruits, one of which he makes out to be "a species of almond," but somewhat qualifies his determination when he further characterises it as "remarkable in the opinion of the most intelligent persons, as bearing no analogy in size to any recent specimens of this nut discoverable in any quarter of the globe." This particular specimen was the property of Sir Joseph Banks, and is probably identical with one of the two figured nearly twenty years later by James Parkinson, a London surgeon, the author of "Organic Remains." By this time the fossils, through Sir Joseph Banks and Mr. Douglas, had become the property of the British Museum. In the "Organic Remains," the green figs and the species of almond become allied to the coco-nut, Parkinson suggesting that they may, perhaps, be referred to the genus *Cocos*.

A year before the publication of Douglas's "Dissertation," Burtin, in his "Oryctographie de Bruxelles," had already described as coconuts some large fossil fruits found in beds of Middle Eocene age at Woluwe near Brussels. Others have since been brought from the same locality, and may be seen along with the Sheppey fossils in the geological department at the Natural History Museum; they are as large as coco-nuts, but broader and more egg-shaped.

None of these fruits received specific names till 1828, when Adolphe Brongniart, in his "Prodrome," accepting Parkinson's generic determination, dedicated the Belgian ones to Burtin, as *Cocos Burtini*, and those from Sheppey to Parkinson, as *C. Parkinsonis*.

In 1840, James Bowerbank, who had accumulated an enormous number of fossil plants from Sheppey, brought out the first and only part of his "History of the Fossil Fruits and Seeds of the London Clay." The first portion of this book is devoted to a discussion of the affinities of the particular specimens above-mentioned and others more or less resembling them. From a careful study of a large number, Bowerbank was led to reject the idea of a close alliance with the coco-nut, and also the name *Pandanocarpum*, applied to some of them by Brongniart, to indicate what he considered to be their great analogy with the fruits of *Pandanus*, the Screw-pine. As Bowerbank points out, the fruits are never aggregated into portions containing several seeds as occurs in *Pandanus*, while on the other hand they closely resemble those of *Nipa fruticans*. In both cases we find the thin outer skin, or epicarp, the fibrous central portion, or mesocarp, and the somewhat indurated internal endocarp, protecting the single seed. Each is in fact a drupe constructed on the same principle as the plum or almond. The fossils never show any indication of the presence of three carpels, either by three pores as in the modern coco-nut or otherwise. To indicate their affinity with the modern *Nipa*, Bowerbank founded the genus *Nipadites* and went

on to describe no less than thirteen species. So close is the resemblance to *Nipa*, that though widely separated from its present area of distribution, there is little doubt but that the Sheppey fruits belong to that genus of Palms. In fact, I was greatly tempted in a recent monograph, which the Linnean Society did me the honour to publish in their Journal, to follow Ettingshausen, who makes them all species of *Nipa*. The suffix "ites" has, however, been much used in palæontological work, and as there must always be some uncertainty in the specific diagnosis of fossils, based, as it often is, on material which would scarcely be considered sufficient in the case of recent plants, I have retained the form in which it was left by Bowerbank.

As often happens when a man makes a special study of small groups of plants, Bowerbank made too many species out of his fossil palm fruits. Some of these are founded on differences which may be merely individual variations or depend on the degree of maturity of the fruit or its position in the head. It is important to remember that, densely packed as they are in a sphere, there is not only considerable variety in the number of sides to the fruit and the development of the angles, but as many never ripen, all stages of abortion may be found, especially near the base of the head, compressed and distorted by the growth of the ripening drupes. There is every reason to believe that the fossil fruits were borne in a similar way. A comparison of some of Bowerbank's species with a number of immature or aborted fruits, taken from a head kindly sent by Mr. Ridley from Singapore, showed that more than one might be thus accounted for. The thirteen can be in this way reduced to six or at the most seven. One or more of these have also been found near Bournemouth and in the Isle of Wight. Three years ago Mr. Clement Reid, of the Geological Survey, made an interesting discovery on the Sussex coast in the Bracklesham Beds, at Bracklesham and West Wittering. In portions of the beach bared by the effect of a recent gale, he saw embedded a number of large fruits which we found on closer investigation to be *Nipadites* of a much larger size than any hitherto found in this country, and quite equalling those already referred to from the Belgian beds. Mr. Reid has since come upon similar specimens at Hengistbury Head, near Christchurch, Hants. The mode of preservation of these differs from that of the London Clay and Belgian fruits, for while the latter have been rendered hard and solid through the infiltration of mineral matter, our recent finds consist merely of a carbonised shell filled with sand, which rapidly crumbles on removal from the damp sea beach, while the carbon film cracks and peels. Hence it is impossible to keep them for any length of time. One, in a fair state of preservation, is now in the Jermyn Street Museum, and another in the British Museum at South Kensington. Fruits almost precisely similar to these have been found in Italy, near Breonio, in Verona.

Though described under a different name, *Palæokeura*, I have no doubt as to their identity with *Nipadites*.

The conditions under which the fossils are found show that, like their modern representative on the shores of tropical Asia, the palms from which they fell grew in the rich brackish mud of the shore or estuary. Associated with the Sheppey fruits are remains of crabs, fresh-water turtles, and a great variety of fruits and seeds, with stems and branches; such a collection, in fact, as characterises the delta of a large river. The great diversity of forms, and the water-worn condition of many of the specimens, indicate a collection brought together from a large area. The Belgian and Italian fruits must have floated some time, as they are often much bored by the teredo, and the associated fossils have a somewhat more marine character than those from the London Clay. On the other hand, the appearance of the fruits which we saw on the Sussex coast gave the idea that they might have grown close by, so uniform was their size and condition. The absence of teredo-borings and water-wear favours this view, but this being the case, we should almost expect to find traces of the stem or leaves of the palm ; these, however, have not yet been found in any locality. Water-worn drift-wood, showing coniferous and dicotyledonous structure, occurred along with the West Wittering specimens.

From the general facies of the flora and fauna in the different beds in which *Nipadites* are found, there is ample evidence, as Bowerbank, Ettingshausen, and Schimper have demonstrated, of the existence of a climate much warmer than the present, and approaching a tropical one, or at least subtropical in character.

A. B. Rendle.

V.

The Bird's Foot.

I.—By Frederic A. Lucas.

I TRUST that the author of the note on the bird's foot, Natural Science for July, page 10, will not consider me "a captious critic" for saying that the statement that "in the great majority of birds. . . . the tendon of the flexor of the great toe is connected by a tendinous slip with the common deep tendon of the other toes" may be a little misleading.

Regarding the groups of birds in a general way, the statement is true, but so far as mere numbers go, the Passeres and Humming-birds, which are schizopelmous (*i.e.*, with the tendon to the great toe unconnected with the common deep tendon), probably contain a majority of species.

Now in regard to the presence of the flexor longus hallucis in three-toed birds, it seems to me that we can bring additional evidence to sustain the proposition that it remains not as a mere rudiment, but because it is functional. In certain lizards—possibly in the majority, or even all—the long flexors of the foot unite beneath the ankle and are worked by a single muscle; and did we know of any bird in which there was no separate muscle for the flexor longus hallucis, this bird would be considered as presenting the primitive arrangement of the tendons.

In the common Chimney Swift of North America (*Chætura pelasgia*) we have a comparatively simple condition of affairs. The deep tendons are united for a considerable distance, and the tendons to III, 2, and IV, 3,[1] arise from a common slip. There is another tendon to II, 1, the total number of separate long tendons thus being four. I, 1, is worked by a separate muscle and tendon arising from the upper inner portion of the tarso-metatarsus, this being present also in other Swifts, in Humming-birds, and in such Passeres as I have dissected.

In another and more typical Swift, *Micropus melanoleucus*, there are separate tendons to II, 2, III, 2, and IV, 2, the deep flexors being, as before, firmly united. In *Trochilus* we have a somewhat intermediate

[1] Roman numerals represent digits; Arabic, phalanges.

condition, the deep flexors being independent of one another, while III, 2, and IV, 3, are, as in *Chætura*, forks of a common tendon, separate tendons running to I, 1, and II, 1. Finally, in a number of passerine birds we have, besides the separate deep flexors, the following tendons, II, 1, II, 2, III, 1, III, 2, and IV, 1, seven long tendons in all. Concomitant with this there are other steps towards increase in what we may call tendinal individuality, for while in the Swifts the tendons playin a groove[2] at the upper end of the tarso-metatarsus, in some, though not in all, Passeres, there are six distinct foramina for the accommodation of the seven tendons.

Now the bearing of these facts on the case under consideration seems to me this : There is an increase in the number and freedom of tendons as we proceed from the lower to the higher birds, and if three-toed birds retain the flexor longus hallucis it is because they stand low in the avian scale and had lost their fourth toe before its tendon became differentiated from the others.

Right here is a point I should like to see explained. Why has *Chauna*—and *Palamedea* also—a thumb and no flexor ? Has it lost the flexor through disuse, or has it, which it has no right to do, grown a thumb after having once discarded that digit ?

Turning back to Mr. Finn's interesting paper in the June number, there are one or two points which invite a few additional words. The great objection to Darwin's remark that "it might be of advantage to a penguin-like bird first to flap along the surface of the water like the Loggerhead Duck (*Micropterus*) and ultimately rise in the air" is that, so far as I am aware, birds which take to water have their flight impaired, and that the transition is from air to water, and not from water to air. The Loggerhead Duck is said to fly when young and do its flopping later in life. The auks have had their wings much reduced in order to use them in a medium for which they were not originally intended, while the great auk lost his power of flight, and eventually his life, by not taking sufficient aërial exercise.

Lastly, is the foot with all toes turned forward a case of reversion for adaptive purposes, or is it a case of survival ? While the supposition that, if birds are descended from reptiles, these reptiles were arboreal in their habits seems to the writer as very plausible, it is also fair to assume that there was a time when all four toes pointed forward,[3] and why may not the cosmopolitan Swifts have retained their toes as they were originally ? Opposed to this, however, is the highly modified condition of the phalanges, and the fact that in *Micropus* the tendons are a little more specialised than in *Chætura*, which has the

[2] *Macropteryx* has a single tendinal foramen, but the members of the genus *Macropteryx*, though Swifts, belong, to the best of my knowledge and belief, to a distinct family.

[3] In support of this assumption is the fact that in the embryo the four toes point forward.

foot of an ordinary perching bird; so for the present we may let the question rest.

II.—By Frank Finn.

There are some points in Mr. Lucas's interesting paper on this subject on which I should like to offer a few remarks.

In the first place, Mr. Lucas considers that the deterioration of flight in birds which take to water is the greatest objection to the derivation of flying birds from aquatic ancestors. This would indeed be a powerful argument were such deterioration universal; but it is not. A surface-swimming bird may on the one hand take to diving, and then is pretty certain to deteriorate in flying power. The Anatidæ form a good example of this, the semi-terrestrial Sheldrakes having larger and broader wings than the more aquatic typical ducks, while these again are better winged than the diving-ducks (Fuligulinæ); and the spine-tailed ducks (Erismaturinæ), which seem to be as aquatic as Grebes, have the smallest wings of all, except the Loggerhead, which is an outlying degenerate form with no very great diving power.

But, on the other hand, a surface-swimming bird may take to flying more than ever. It can hardly be doubted that the Gulls are better flyers than even their Plover-like ancestor, though the Alcidæ have, as Mr. Lucas points out, degenerated.

Similarly, the Steganopodes exhibit, in the Pelicans, surface-swimming forms with powerful wings, and the wing-power is increased in the Gannets, Tropic birds, and especially the Frigate birds; while the diving Cormorants have their wings reduced, and the extinct great Shag (*Phalacrocorax perspicillatus*) was apparently becoming flightless when its fate overtook it.

The Phalaropes present us with an interesting instance of water-birds of recent evolution; they are as yet only surface-swimmers, with wings not noticeably different from those of ordinary littoral sandpipers, and may in the future develop either way.

Thus, the transition is rather, in the first place, from land to water, and the bird may either become more aërial or more aquatic in consequence of the change of habit.

Similarly, birds which are markedly terrestrial are either good flyers, as Plovers and Sand-grouse, or exhibit a tendency to degeneration in flight just as marked as that of some aquatic birds. Instances of this are, of course, the Ratites, and such Carinates as *Ocydromus* and *Stringops*. The Gallinæ and Crypturi are also usually poor flyers, though none are yet quite flightless. *Stringops*, be it observed, must have lost its arboreal habits before it became flightless, and we find that the genera of Parrots which are probably most nearly allied to it, *Pezoporus* and *Geopsittacus*, are already terrestrial. Arboreal habits tend to preserve an average type of wing, flightlessness being obviously disadvantageous in such case, and also marked length of

wing feathers.[4] And this average wing is particularly characteristic of the Passerines and of most of their allies, which, as has long been known, present a general structure most marked by the absence of special peculiarities. In certain respects, indeed, their specialisation fairly entitles them to their usually conceded place at the head of the avian tree; but their broad outlines are those of a simple and primitive form. In this respect, as I pointed out in my previous paper, they show some analogy to the Primates among Mammals.

This brings me to my second point. Mr. Lucas suggests that the first birds might have had all their toes pointing forward, and that the Swifts may have retained this arrangement. But the fact, which he points out, and which I implied in my paper, that this arrangement is not universal even with them, tells against this view, as he admits. As to the embryo, I must say that in some nearly mature embryos of the Common Swift of Europe (*Cypselus apus*) which I examined, the hallux did not seem to me distinctly directed forwards.

Besides this, it is noteworthy in this connection that a type of foot with opposable toes is very likely to be developed in consequence of arboreal habits. The Primates almost universally, the Opossums, and *Lophiomys* in the Rodents, have opposable halluces among the Mammals; and though the grasping foot of the Chamæleons is not exactly like that of a perching bird, it shows that a very arboreal reptile can develop a prehensile foot with opposed digits; and I can see no reason why this should not have taken place with the primitive bird-reptile before it acquired the power of flight.

III.—By P. Chalmers Mitchell.

Owing to the opportunities afforded me by the kindness of my friend, Mr. F. E. Beddard, the Zoological Society's Prosector, I have recently been able to devote a considerable amount of attention to the anatomy of the bird's foot, and I am glad to respond to the invitation of the Editor to say something on the interesting discussion raised by Mr. Finn's paper in the June number, and by the editorial note in the July number of Natural Science.

There are two questions which have an important bearing upon each other, but which must be argued out separately. Mr. Finn argued for his view that three-toed birds, so far as the toes are concerned, are degenerate descendants of four-toed birds, and that the avian stock probably arose from an arboreal four- or five-toed reptile. In support of his argument, he brought together the cases in which apparently three-toed birds have been shown to possess a rudimentary fourth toe. In the Natural Science note it was pointed out that

[4] A typical form would be the Jay. Is it not possible that the flight by single violent flaps alternating with sailing often affected by this bird and others similarly winged may represent the earliest form of flight, originating in the effort of a parachuting animal to avoid falling?

even in those cases where the first toe is absent there is present a distinct flexor muscle corresponding in its position and relations to the flexor longus hallucis, the long flexor of the great toe. The tendon of this muscle has, of course, no great toe in which to be inserted, and unites with the common tendon of the deep flexor which splits up to work the other three toes. The writer pointed out that the existence of this muscle appeared to be strongly confirmatory of Mr. Finn's view. Unless three-toed birds originally were in possession of a hallux, why should they have a flexor hallucis? He proceeded to point out that this, however, cannot be taken as the survival of a meaningless vestige. For, in the great majority of avian groups (I accept Mr. Lucas's emendation, and quite agree with him that the words of the note were expressed so that a careless reader might suppose that individuals or species were meant), the flexor tendon of the hallux, before running to its insertion upon the hallux, gives off a stout slip to the common deep tendon of the other toes. Apparently it is this functional part of the muscle that is preserved. Were there a case of a Passerine where the hallux is absent, then we could see whether a functionless rudiment would be retained; for in Passerines there is no slip from the longus hallucis to the common tendon of the other toes.

Mr. Lucas brings forward interesting considerations directed to the view that in birds there is a progressive tendency towards the individualisation of these tendons. He suggests that, as is the case in many existing Lizards, so possibly in primitive birds a single muscle had many tendinous insertions, and that later on the muscle gradually broke up till more and more of the tendinous insertions came to have separate muscular bellies. He suggests, therefore, that in the case of three-toed birds they may have lost their toe before the muscle became individualised, and therefore he thinks that this condition of the tendons means that the birds are low in the avian scale, and therefore also, he concludes that the note does not supply an argument in favour of Mr. Finn's view. The three-toed birds, according to him, do not show evidence of the modification of their foot from a specialised four-toed foot, such as would be possessed by an arboreal form.

The matter is so interesting that I shall elaborate the argument more fully, as I am convinced that it is sound. In a typical four-toed bird there are two deep plantar muscles. The belly of the flexor longus communis lies most closely applied to the posterior surface of the shaft of the tibio-tarsus. At its upper end it spreads out fan-like, and takes origin sometimes continuously, sometimes by a series of digitations, from the upper end of the tibio-tarsus, with occasionally a slip from the femur. Its single tendon runs through a bony canal in the posterior part of the ankle-joint, and then runs down the posterior face of the shaft of the tarsus-metatarsus, near the lower end of which it splits up into a tendon for the second, third, and fourth toes. The flexor longus hallucis lies externally to the belly of the flexor

communis. It is a rounder and narrower muscle, entirely distinct from and unconnected with the flexor communis above the ankle-joint, and arising by a single head or by two heads from the external condyle of the femur, and from the intercondylar region. Its single tendon of insertion passes through the ankle-joint, generally through a separate bony canal which is placed more laterally than the bony canal of the flexor communis. Below the ankle-joint the tendons of the two muscles lie side by side, but in their course down the shaft of the tarsus-metatarsus the tendon of the flexor hallucis crosses over the tendon of the other, and in so doing (as has already been mentioned) except in the Passeres and Humming-birds, it sends a strong vinculum to the communis tendon, and then runs on to its insertion in the hallux. Now whatever may have been their original connection, these two muscles are as distinct and individual as possible. In the case of an ordinary three-toed bird like the Oyster-Catcher (*Hæmatopus ostralegus*), the two muscles are as separate in their origin and course as in a normal four-toed bird ; the only difference is, that with the loss of the hallux, the slip of tendon which runs from below the vinculum to the tendon has disappeared. The facts almost irresistibly bear out the conclusion that such a three-toed bird has been derived from a four-toed bird not of a primitive character, but with a foot like existing normal four-toed birds. But evidence—one is almost tempted to call it experimental evidence—is furnished in favour of this view by the anatomy of the foot of *Pterocles.* Whether the Sand-grouse be considered a pigeon or a gallinaceous bird, most will agree that it is in the present condition of losing its great toe. In *Pterocles alchata* there is no trace whatever of the slip from the vinculum to the minute toe. But above that the two tendons, and above the ankle-joint the two muscles, are perfectly distinct ; the flexor longus hallucis arises by two heads from the femur, while the flexor communis spreads out over the upper end of the tibio-tarsal shaft. Here is a case of recent degeneration of a hallux and the production of the same type of foot as in ordinary three-toed birds. It seems to me absolutely conclusive in favour of Mr. Finn's view.

But Mr. Lucas's remark about *Chauna* and *Palamedea* calls up another exceedingly strong piece of evidence. *Chauna* has no flexor tendon to its hallux, but in other respects the relations of the flexor longus hallucis down to the vinculum are precisely similar to those of four-toed birds. Mr. Beddard and I recently had an opportunity of dissecting *Palamedea*, and the results of our observations, communicated to the Zoological Society, will shortly be published in its *Proceedings.* We found that there was a separate tendinous slip to the hallux. So here again is a case showing that the mere suppression of the tendinous slip from the vinculum to the hallux changes a four-toed foot into the type of structure found in three-toed birds.

VI.

Notes from the British Association, 1894.

THE British Association is always a medley, and this characteristic is very apparent in the addresses delivered before it and the papers read at it. There are always some people who have something to say and some who have to say something. There are those who have manner without matter, and those who have matter without manner. Its great mission is to interest the public in science. The general public go there to be as scientific as they can. The scientific public go there to frivol as much as they may. It serves many useful purposes, but among these the least is to bring new scientific results before the scientific public. But although one need not expect any large bulk of novel and interesting scientific matter, there are always some interesting messages from the scientific world to the public, and not a few matters of general scientific interest.

We propose to give here, not an account of the meeting that shall pretend to any completeness, but notes on such matters as seemed of special interest to our representatives at the sections. As we have dealt elsewhere with Lord Salisbury's address, we shall pass straight to the sectional meetings.

Neglected Mineralogy.

The President of the Geological Section represents a neglected branch of science, and Mr. Lazarus Fletcher made excellent use of his opportunity in a direct plea for mineralogy by pointing out the lack of opportunity for the study of it in this country, and in an equally cogent indirect plea by the brilliancy of his own exposition.

Having given a Homerically imposing alphabetical list of living German scientists, from Arzruni to Zirkel, whose names are familiar as distinguished mineralogists, Mr. Fletcher went on to contrast the wealth of educational and research equipment of Germany with the apathetic treatment which mineralogy is receiving in the British Isles. In every German university there is a professor of pure mineralogy, while other continental nations are not far behind Germany in their practical appreciation of the subject. But here, throughout Great Britain and Ireland, the imposing number of the professorships in the science amounts to *two*; and in only one of the two universities is the subject allowed to rank with other sciences in the examinations for degrees.

University extension in a mineralogical direction was the theme of Mr. Fletcher's interesting address, and surely such an appeal was never more needed than at the present time when the study of minerals is being more than ever pressed by other countries, and must be undertaken by almost every geological student. Such has been the indifference felt towards mineralogy, that the last report on the progress of the science submitted to the British Association was that given sixty-two years ago by Dr. Whewell, shortly after he had resigned the professorship of mineralogy in the university of Cambridge. It was therefore incumbent upon the president of Section C, as a mineralogist, to give some account of the progress of the science since that date, and of its present condition: this duty Mr. Fletcher performed in the earlier part of his address in a masterly and captivating manner.

The audience was rapidly conducted over the recent history of crystallographic methods and theories up to the parallelepipedal and other arrangements of points which have been devised to account for the structure of crystals, special emphasis being laid upon the notation and methods introduced by the late Professor Miller, of Cambridge. This was followed by a brief survey of the development of optical and physical research as applied to the study of minerals; by a sketch of the remarkable progress of mineralogical chemistry during the last sixty years; and finally by an enumeration of the more important instruments which have been invented and improved during this period and which have enabled the mineralogist to extend his investigations in every direction, and have helped to raise mineralogy to the position of an exact science. No one has by his labours contributed more to this result than the late Professor Mallard, of Paris, whose lamented death last month was alluded to by the President as depriving mineralogy of her greatest philosopher.

If much of the address dealt with things that were absolutely unknown to the majority of his audience, Mr. Fletcher's argument was only the more strengthened, for where, under the present conditions, could they be expected to have received any instruction in the refinements of modern mineralogy, or to have learnt that there is a real science of minerals?

We hope that this address will open the eyes of many people. The words quoted from the Report of Dr. Whewell are strictly applicable to the present day; they related to the lack of "interest with which mineralogy, as a branch of natural philosophy, has been looked upon in England. Indeed, this feeling appears to have gone so far that all the general questions of the science excite with us scarcely any notice whatever. But a more forward and hopeful spirit appears to have prevailed for some time in other countries, especially Sweden, Germany, and more recently France." And yet "the step to which mineralogy owes the best portion of its scientific character was made by an Englishman, the Doctrine of Definite Proportions,

and if Englishmen seriously propose to themselves the task, we are justified by the history of Science in asserting that none are more likely than they to solve the great problem of mineralogy which now offers itself—the connection of chemical composition and crystalline form." Such were the words of Dr. Whewell in 1832.

Indeed, we have only to recall the additional names of Wollaston, Brewster, and Miller to remind ourselves that much of the most original work of the century has been done by English mineralogists. An appreciative reference, in Mr. Fletcher's address, to the highly philosophical teaching of Professor Maskelyne on crystalline symmetry, shows that the true spirit is by no means dead among us even in the most abstruse branch of the science; and Mr. Barlow has recently made important contributions to the same subject. Professor Renard, of Gand, in seconding a vote of thanks, declared that no one had done more to elevate the respect already felt abroad for British mineralogy than the distinguished President himself, and referred especially to his admirable "Introduction to the Study of Minerals," which was published in the form of a guide to the vast collection, of which he has the charge, at South Kensington.

Anthropology.

Sir William Flower performed a most useful task when, in his presidential address, he reviewed the past history and the future prospects of anthropology. As a separate branch of science anthropology is so new, and on many of its sides is so largely aided and furthered by the amateur, that it was well worth while pointing out that the scientific training and equipment for the scientific study of anthropology is as great as those required in any of the older sciences. "It invokes the aid of the sciences of zoology, comparative anatomy, and physiology, in its attempts to estimate the distinctions and the resemblances between man and his nearest allies, and in fixing his place in the scale of living beings. In endeavouring to investigate the origin and antiquity of man, geology must lend its assistance to determine the comparative ages of the strata in which the evidences of his existence are found, and researches into his early history soon trench upon totally different branches of knowledge. In tracing the progress of the race from its most primitive condition, the characteristics of its physical structure and relations with the lower animals are soon left behind, and it is upon evidence of a kind peculiar to the human species, and by which man is so pre-eminently distinguished from all other living beings, that our conclusions mainly rest. The study of the works of our earliest known forefathers—'prehistoric archæology,' as it is commonly called—is now almost a science by itself."

Towards the conclusion of his address the President gave an account of the conclusions arrived at by the committee appointed by Mr. Asquith to inquire and report upon the "anthropometric"

methods of registering criminals. He expressed complete agreement with their recommendation of a modification of the French system of measurements, with the addition of Galton's "finger-print" method.

The Antiquity of Man.

It is interesting to watch the gradual change of opinion as to the antiquity of the human race, and to compare what is said at Oxford in 1894 with discussions of the same question at former meetings of the British Association. At one time the debate was largely one for theologians to take part in, and personalities were freely used; now a joint meeting of the anthropologists and the geologists to discuss the Plateau Flint Implements of North Kent leads to a thoroughly scientific and calm review of the whole question. It was unfortunate that age and illness should have prevented the Nestor of our geologists, Professor Prestwich, from taking part in so interesting a debate, and it was unfortunate also that Mr. W. J. L. Abbott, who has done so much good work in the Plateau Gravels of recent years, and whose interesting paper on this very subject will be remembered by our readers, should also have been too unwell to attend; otherwise nearly all the leading authorities took part in the debate.

Professor T. R. Jones opened the discussion on behalf of Professor Prestwich, maintaining his well-known views as to the vast antiquity of the rude implements found on the higher plateaus. Mr. Whitaker doubted whether many of the supposed implements exhibited at the meeting were really of artificial origin, but acknowledged that some were certainly genuine. Most of these had been picked up on the surface, not dug out of undisturbed gravels. He questioned whether the discovery of implements in the "clay with flints" was any evidence of age, for the clay with flints was still in process of formation. Mr. Montgomerie Bell thought that he could trace a continuously improving series of implements in the different deposits of Kent, but Sir John Evans doubted whether Kent was really the cradle of the human race.

Dr. Hicks spoke of the relation of the implement-bearing gravels of Middlesex to the glacial and cave-deposits of North Wales, and insisted that man was pre-Glacial. Professor Boyd Dawkins pointed out that rude workmanship was no satisfactory evidence of antiquity, as rough or finished implements would be used according to the purpose for which they were required. General Pitt-Rivers spoke on the gradual evolution of implements of different types. He did not consider the mere occurrence of a bulb of percussion as sufficient evidence of artificial origin, and doubted the value of the evidence as to the implements having been found in place in the plateau deposits. Mr. Clement Reid questioned the extreme antiquity of the implements, pointing out that careful search had led to no discoveries in undoubted Pliocene and pre-Glacial deposits. Colonel

Godwin-Austen suggested that much of the apparently artificial chipping of the flints had originated through the rubbing together of stones in masses of talus and mud. On the whole, then, the general sense of the meeting was opposed to granting any extreme antiquity to the early dwellers on the former plateau of Kent.

Forestry.

Professor Bayley Balfour, in his capacity as President, addressed the Biological Section on the subject of Forestry. In many respects this address was the most important communication that was delivered to the public in any of the Sections of the Association.

Two reasons have interfered to prevent the English people paying necessary attention to the afforestation of our land. The first is the abundance of coal in this country. For many years all practical men have regarded the supply as almost inexhaustible, and it is only now that the warnings of experts in mining and geology have been accentuated by the actual exhaustion of many of our coal-fields that there is a possibility of getting the public to believe that England may soon have to revert to the partial use of charcoal. The next reason is that the continuous extension of our commerce and our territory has opened up for use series of forest-clad areas, while the woods and forests of the continent have by themselves been nearly able to supply our demands. But the increasing scarcity and price of imported timber is now awakening our eyes to the enormous waste of our timber-growing capacity. This waste is in two directions: the forests we have, are, from want of trained experts, in many cases very badly managed; immense areas, well adapted for growing timber, and ill adapted for any other crops, are left barren.

Besides the direct economic advantage of increasing the productiveness of the land, there are many other advantages that would accrue were the course urged by Professor Bayley Balfour and others to be adopted. There would be a great increase in the employment of labour; a very large part of at least the first steps in afforestation would demand unskilled manual labour, directed by a comparatively small staff of experts. We believe that the London *Daily Chronicle* has already advocated this as a field for that State employment of the unemployed which is believed by many to be at once prudent and inevitable.

The extension of woodland and forest would increase not only the natural amenity of our country, but it would appeal directly to all naturalists and lovers of animals and plants, in that it would provide an enormous increase of area suitable for many of the most attractive wild animals and plants that can live in this country. Professor Bayley Balfour made a practical suggestion to aid the attainment of his object. "There are tracts which without damage to the natural beauty, and without depriving in any sensible degree the people of their privileges of recreation they prize so much, might

be and should be dealt with as forest cultivated on scientific principles. These might serve as instruction areas, showing all that is best for the information of foresters. The creation of some such experimental teaching stations in State forests is one of the essentials for forestry in Britain. I would go further, and say that the area of State ownership should be increased to the extent of the establishment of forest stations, of an acreage sufficient to allow of a satisfactory rotation, in other parts of the country as centres of instruction." "In a practical science like forestry an increase of sound technical knowledge can only be possible when facilities for practical instruction are provided. I would, therefore, ask the President of the Board of Agriculture to consider what I have just said with regard to State forest experimental areas. These cannot, of course, be created by a stroke of the pen, but the initiative for their formation would naturally come from the Board of Agriculture." "It appears to me that while we must obtain from the Government the institution of sylvicultural areas for practical instruction, our best chance of success in acquiring the necessary endowment for the rest of the teaching lies in the line of combination between the Board of Agriculture and the County Councils, with, it may be, aid from private benefactors. But if we were to draw financial support from County Councils or from private sources, we must as a first step towards this make known, more thoroughly than it is, the nature of the national interests involved. We must disabuse landowners, land agents, and practical foresters of the notion that forestry consists in the random sticking in of trees, which anyone, no matter how unskilled, may accomplish. We must bring home to the people's minds that in science is to be found the only sure guide to proper timber growing, and that scientifically managed forests are alike a profit to the producer, a benefit to the community of the region in which they are reared, and a source of national wealth. Once we have got so far as to create this opinion, the funds for as extended a scheme of forestry education as may be necessary will, I venture to think, be forthcoming."

Turning from the duties of the nation to the duties of botanists, Professor Balfour urged upon them the necessity of paying more attention to the subject, for it is from the ranks of botanists that the directors of future schemes of afforestation and the teachers of practical foresters must come.

The Nucleus in Plants.

The next subject of botanical interest we shall mention is a very technical one, and a subject in which none but the scientific public could possibly take an interest. At one of the meetings of the Biological Section Professor Strasburger gave an admirably clear account of his researches on the periodic variation in the number of chromosomes in the nuclei of plant cells. After discussing the

probable history of sexual and asexual reproduction in plants, Professor Strasburger went on to speak of plants which exhibit alternation of generations. In these forms it is invariably found that in the nuclei of the germ-cells of the asexually produced individuals, a definite number of chromosomes is always present when the nucleus is in an active stage. This number appears, at any rate in the case of the lower cryptogams, to be twelve. Hence it follows that a fertilised ovum contains twenty-four. This latter number characterises every cell in the body of the sexually produced individual up to a certain point, *i.e.*, the appearance of the egg or spore mother-cells. At this point a reducing division sets in, and the number of chromosomes in every spore formed is lessened to half its original number *i.e.*, twelve.

The exact nature of this reducing division is not yet evident.

Carrying these observations further, he finds that in the higher phanerogams, where the sexual generation may be suppressed, the same sudden reduction from sixteen to eight chromosomes occurs previously to the formation of the germ-cells. From these facts he concludes that the reducing division in these plants marks phylogenetically a more primitive condition in which alternation of generations obtained. Professor Bower is translating Professor Strasburger's paper, and the full text of the translation will appear in a forthcoming number of the *Annals of Botany*.

Physiology.

Professor E. A. Schaefer's presidential address derived additional interest from the circumstance that this is the first year since 1847 that there has been a separate section for physiology. While pointing out that the enormous increase in England of those devoting themselves to physiology as a special science had led to the reappearance of this as a separate section of the Association, Dr. Schaefer suggested that it might not be advantageous to have one at every meeting of the Association. "Physiology is, above all things, a practical science. It requires laboratories and means of demonstration. Physiologists are rarely satisfied with the opportunity of hearing and reading papers; but require that, as much as possible, the actual methods of research employed should be capable of demonstration. By this I am not to be supposed to advocate the demonstration of experiments upon animals, for there are very many subjects in physiology which can be both worked at and illustrated in a manner involving in no sense whatever the word vivisection."

We differ from Professor Schaefer's conclusion. To very few of the sections would his words about "practical science" and so forth not be applicable, and for the purposes of demonstration and explanation of the methods of research, there are special gatherings of specialists. In the case of physiology, there is that very energetic Physiological Society, to which Professor Schaefer referred, and

which meets in the physiological laboratories of its members for the precise purposes to which Professor Schaefer alluded. There are subjects galore on which papers can be read and discussions raised without the necessity of laboratory demonstrations. In many respects, however, we cannot help regretting the breaking up of the section of biology into separate sub-sections and sections. It tends more and more to stamp the Association with that isolated specialism which is inevitable in modern science, but which carries with it great disadvantages. If the zoologists meet by themselves in a zoological laboratory, the physiologists in a physiological, and the botanists in a botanical, the odds are that the subjects they discuss will be of a nature to be unintelligible or uninteresting to those of another specialism. We imagine that a botanist or a zoologist would gather little from those demonstrations of the methods of research in a physiological laboratory which Dr. Schaefer advocated. What we want at the meetings of the British Association is an increase of organised discussions of subjects which lie on the borderland of the different branches, and of the different branches of these branches.

To take an example, one of the subjects of Dr. Schaefer's address was recent discoveries about the cell, and specially about the nucleus and the centrosome. Dealing with the researches upon the centrosome of the physiologist Heidenhain, the President described it as one of the obscure subjects of which, though supremely important, physiology knew next to nothing. Now, in the Biological Section, Professor Strasburger read a paper on the nucleus and centrosome in plants, while Dr. Van Beneden discoursed on the same topic from the point of view of a zoologist. Is not this a lamentable instance of failure to take advantage of the opportunity of joint discussion ? Very many similar subjects might be mentioned. For instance, at the present time there are different and novel views of the nature of heredity and of the structure of protoplasm propounded by botanists, physiologists, and zoologists. And elsewhere in this article we allude to similar lack of concert on subjects of Geographical Distribution and Darwinian Theory. We are certain that well organised discussions of these held by biologists from all the sub-sections of biology would prove not only of surpassing interest but of great value. Although the physiologists do not intend to meet as an independent section next year, yet the botanists promise to carry on the evil.

Natural Selection.

The most interesting discussion in the Biological Section was confined almost entirely to zoologists. Professor D'Arcy Thompson boldly threw down his gauntlet by stating his doubts and difficulties in accepting "Darwinism." He could not see how many forms and colours of animal life were to be explained on the simple hypothesis of Natural Selection, even assisted by Sexual Selection; and he instanced the brilliant colours of humming-birds and the logarithmic

spiral of a coiled shell, whether foraminiferal or molluscan, as due rather to physical laws governing growth than to any advantage which they might give their possessors in the struggle for existence. The ingenious speculation that the pear shape of a guillimot's egg is derived from the protection that it affords to the egg from falling off the rock ledge, did not find favour with Mr. Thompson, who pointed out that the same shape was found in eggs, such as the plover's, laid on the broad flat ground. This shape he ascribed to the mechanical pressure exerted by a narrow oviduct on a relatively large egg.

Professor C. V. Riley, the eminent entomologist of Washington, was prevented by the strict time-limits from properly presenting his suggestive conclusions derived from a study of social insects. This we hope partially to remedy by publishing his valuable paper *in extenso.* Broadly put, his opinion was that Natural Selection was an insufficient or impracticable cause in the evolution of the varied groups of individuals met with in an insect colony, and that much was due to the direct action of environment, notably of food. These are views to which much attention has already been directed in our own columns.

Professor J. Berry Haycraft, who felt constrained to describe himself as a physiologist, but not a biologist, raised a most interesting objection to the views held by Weismann as to the part played in evolution by sex. He pointed out that the conjunction of two parents, instead of promoting variation, restrained it and kept it near the mean. This, he said, was proved by the statistics collected by Francis Galton; without sex, we should have no genera or no species, only individuals. This interesting communication will appear in a future number of Natural Science.

Dr. F. A. Dixey contributed to the discussion a detailed account of the evolution of mimetic characters in certain butterflies, without, however, very clearly showing how the gradual stages that led up to the completely mimicking forms could have been due to any struggle for existence or any protective advantage.

Professor H. F. Osborn, of Columbia College, New York, entitled his paper, "Certain Principles of Progressively Adaptive Variation observed in Fossil Series." A large part of it, however, was devoted to emphasising the necessity for analysing variations according to their character and the period of ontogeny at which they occurred. Thus, he divided all variations into Palæogenic and Neogenic, the former being instances of reversion, and therefore not entering, like the latter, into progressive evolution. Neogenic variations may again be divided into four classes :—(1) Gonogenic, due to variation in the original ovum or spermatozoon before their union ; (2) Gamogenic, due to the union of diverse sexual elements ; (3) Embryogenic, due to the influence of environment on the developing embryo; (4) Somatogenic, due to the action of habits or changed environment on post-

embryonic stages. Mr. Osborn pointed out that a study of the development of variations through a long lapse of time showed that similar structures might be reached by quite different roads, as instanced by the molar and pre-molar teeth of *Anchitherium.* He seemed inclined to attribute this more largely to direct mechanical stimulus acting towards the same given end, than to the action of Natural Selection.

After these various onslaughts, the discussion promised to be lively. But Professor Ray Lankester and Professor Poulton, who were the chief supporters of Natural Selection, dealt with the obvious misapprehensions of Darwinism in the papers of those who spoke against Natural Selection rather than with the details of their objections. While we have the strongest sympathy with those who point out the astonishing misconceptions of Darwin's own views, which, unfortunately, are so prevalent with those who bring forward difficulties in the theory of Natural Selection, we think that the attitude adopted by the supporters of Darwinism is unfortunate. For the general public are even more ignorant of Darwin's own writings than those who urge scientific objections to his theories. The impression left on the minds of the camp-followers of science is, that the defenders of Natural Selection are more concerned to maintain an orthodox interpretation of the Darwinian scripture than to do battle about the actual facts.

Mr. Pycraft followed this discussion with a valuable account of the wing of *Archæopteryx*, which gave rise to an animated discussion, in which Dr. Sclater, Professor Ray Lankester, Dr. Hurst, Mr. Finn, and others took part. As, however, this paper will shortly appear in our pages, further description is now unnecessary.

The Distribution of Animals.

This subject, in one or other of its varied phases, was dealt with in many of the Sections, and it seems a pity that some arrangement could not have been made by the organising committees to enable those interested to meet and discuss the question at the same place and time. Professor W. A. Herdman endeavoured to accommodate himself to the situation by reading his valuable Report on the Marine Zoology of the Irish Sea before the Geological as well as the Zoological Section. The result, however, was that in neither place could it receive the attention which all felt that it deserved. The Committee, of which Professor Herdman is both Chairman and Reporter, has paid particular attention to the deposits now forming on the floor of the Irish Sea. It has mapped out the distribution of the various sands, muds, shell-conglomerates, clays, and nullipore deposits, and has endeavoured to see how they are connected with the older geological formations of the sea coast and the submarine deposits lying off the shore. It has further come to the very important conclusion that the nature of the deposit is the most important of the

various factors that determine the distribution of animals over the sea-bottom within one zoological area. It is proved to be more important than mere depth; while the same locality may vary so much from time to time in the temperature, the salinity and the transparency of the water, that these factors, within certain limits, have far less influence upon the fauna than has the nature of the deposit. In the discussion that took place on this paper, Mr. F. A. Bather pointed out how important the study of these modern conditions was to the geologist who attempted to correlate strata by means of their fossil contents; and he suggested the possibility that at some future epoch, when the floor of the Irish Sea had become dry land, these absolutely contemporary deposits might come to be regarded as a series of successive formations, each characterised by its peculiar "zonal" fossils. A similar state of affairs in the Silurian rocks of Gotland has actually given rise to much heated controversy.

Among the more interesting discussions in the Geological Section was that which arose out of Dr. Hicks' paper on The Homes and Migrations of the Earliest known Forms of Animal Life as indicated by Recent Researches. Few of the speakers, however, seemed to confine themselves to the real questions at issue, a fact which may have been due to a want of clear understanding of Dr. Hicks' position. We hope that our readers will be able soon to form their own opinions on the subject, as this interesting communication will appear in a future number of NATURAL SCIENCE.

In the Zoological Sub-section Dr. Otto Maas, whose paper on "Some Problems of the Distribution of Marine Animals" (NATURAL SCIENCE, vol. ii., p. 92) our readers will remember, gave an interesting account of "temperature as a factor in the distribution of marine animals." It was pointed out by Messrs. H. N. Dickson and W. Garstang, during the discussion, that much variation of fauna, apparently due to variation of temperature, might really be due to an influx of water from other sources and to the consequent introduction of foreign forms of life, or the change of the chemical and physical characters of the water. Most speakers, however, seemed to think that so important a paper should be published before it could be discussed. We have much pleasure in announcing that the complete work will appear in an early number of NATURAL SCIENCE.

These seasonal movements of the water in the North Sea and in the Atlantic, which have so much bearing on this subject, were discussed more fully by Mr. Dickson before the Geographical Section. Here also there was a discussion on the distribution of African fish, and, finally, it was here that Dr. John Murray thought fit to read his important paper on the Geographical and Bathymetrical Distribution of Marine Organisms. In the course of this he attempted to demolish the Antarctic Continent of Mr. H. O. Forbes, a question on which geologists and zoologists, no less than geographers, might well have been asked for their opinion.

Maxim's Flying Machine.

Although artificial flying is a problem to be settled by engineers rather than by zoologists, it is a subject which always must have a special fascination for zoologists. It was therefore natural that a number of zoologists should have deserted to the mechanical section to hear Mr. Maxim.

The subject of his paper is of fascinating interest, and the accounts of his experiments, which have extended over some years, were also exceedingly interesting. As might be expected from a man of Mr. Maxim's inventive genius, something has come out. The first is a steam engine of hitherto unrivalled lightness compared to the enormous power it can develop, and a system of aeroplanes which, as the author claims, has enabled him to be the first to fly—not for long, it is true, but that is, after all, only a question of time. The question of landing safely has not yet been solved, and there seems to be some difficulty, as the machine ceases to maintain itself at anything under thirty-five miles an hour.

While Mr. Maxim has not yet got a perfect flying machine, he certainly has advanced considerably towards the solution of the problem ; though it may be questioned whether he has, in using the system of aeroplanes, hit on the best plan, that is if one of the conditions is that the flying machine has to be used for warlike purposes, and not for rapid transit. Perhaps something of the kind suggested by Lord Kelvin is more likely to effect this object.

Lord Kelvin and Lord Rayleigh had both ridden on the machine, and expressed themselves highly pleased with the extremely skilful way in which Mr. Maxim had overcome the various difficulties the condition of lightness in everything had imposed upon him. Mr. Langley, a notice of whose recent publication, on "The Internal Work of the Wind," appears on our next page, sounded a note of warning about the liability of the machine to turn, which, coming from such an authority, must not be disregarded.

Many other points of interest were raised at the meeting, which we have not space to discuss. We hope to recur to some of them on a future occasion.

SOME NEW BOOKS.

THE INTERNAL WORK OF THE WIND. By S. P. Langley. Published by the Smithsonian Institution, Washington, 1893.

To understand how birds fly, and to imitate them ourselves, were primitive ambitions of the race; and ambitions, in spite of Mr. Maxim's aeroplanes, they remain. By severe exertion Lilienthal was able, for a few moments at a time, to develop an ascensional force equal to something less than his own weight. The condor and other large birds poise themselves for hours in mid-air, rising, falling, and progressing with no apparent effort, and without the slightest vibratory movement of their expanded wings. The omission of some important factor in the theory can alone account for so great a discrepancy in the result. That this factor is in some way *the wind* has been long suspected; but the idea, sometimes held, that a uniform current (however strong) can for long assist the soarer, is at variance with the elementary principles of dynamics. In order that the bird may rise like a paper kite there must needs be something to take the place of the string. In a uniform current there is no force available for the purpose except the bird's inertia during the first few seconds after leaving the ground; but that birds do use their inertia, at all events in this limited way, is proved by the observation that many of them are unable to soar without facing to windward at starting. If, however, the current is *not* uniform, the limitation disappears, for in passing from a quicker to a slower current, or *vice versâ*, inertia again comes into play. The question, therefore, remains, do such inequalities exist, and if so, are they adequate to account for the phenomena? Professor Langley's paper answers both parts of the question affirmatively, and it is not too much to say that his experiments constitute by much the most important advance yet made in the theory of aërial flight. "In the ordinary use of the anemometer," he writes, "the registry is seldom taken as often as once a minute." His first experiment was to place an ordinary Robinson anemometer on a mast fifty-three feet high and connect it electrically with the chronograph of the Allegheny Observatory, so as to register every 25 revolutions of the cups. The results are drawn out in a diagram with abscissæ = Time, and ordinates = Wind velocity. In a period of about five minutes, taken at random from the diagram, the wind velocity is seen to have fallen from 13 miles an hour to 9, risen to 17, fallen to 14½, risen to 15, fallen to 14, risen to 19, fallen to 13, risen to 18½, fallen to 14, and risen to 17. This degree of variability in "the wind" is remarkable enough, but Professor Langley's research did not stop here. He proceeded to lighten the cups and to make an automatic record of *every revolution.* The resulting curve often exhibiting nine or ten cusps within a single minute, and deviations of 30 or 40 per cent. on both sides of the line of mean velocity, reveals

a state of things hitherto quite unsuspected either by meteorologists or theorists on flight. Extreme variability seems to be equally characteristic of strong and of gentle winds, nor is there any reason to suppose that a still lighter instrument recording still more frequently would not bring to light further inequality. It is quite likely that "the incessant alterations which, it appears, make 'wind,' are due to impulses and changes which are preserved in it, and which die away with considerable slowness." What we should like to know more of is the change (if any) in *direction* which accompanies the change in velocity. Is the "structure" of the wind mainly undulatory or mainly vertical? that is to say, are the variations observed at a fixed point due to the passage of "ripples" or minute "cyclones"? Upon the answer to this question, it seems as if the future of what Professor Langley calls "Aërodromics" will largely depend.

R. C. GILSON.

PLANT CLASSIFICATION.

UEBERSICHT DES NATÜRLICHEN SYSTEMS DER PFLANZEN. Zum Gebrauch in Vorlesungen für Anfänger. By E. Pfitzer. 8vo. Pp. iv., 36. Heidelberg, 1894. Price 1s.

PROFESSOR PFITZER'S "Übersicht des Natürlichen Systems der Pflanzen," designed primarily for students attending his course of lectures for beginners, will, we doubt not, be found useful at centres of learning other than Heidelberg. It is a small octavo volume of thirty-six pages, with one side blank for notes, and contains a sketch of plant classification. In a few lines are given the chief characteristics of the great groups and divisions, and, *à propos* of these, we cannot fail to notice the changes which have occurred in this branch of the science during the last few years. Those of nomenclature, though necessitating the loss of well-known terms, indicate an advance, in that real and not imaginary differences are now expressed. Instead of the Phanerogams and Cryptogams of the text-books of not many years ago, we now find plants ranged under the three great groups, Siphonogamæ (equals Phanerogams), where the egg, or oosphere, is contained in an ovule and fertilised by the agency of a pollen-tube; Archegoniatæ, where the egg is contained in an *archegonium* and fertilised by an independent cell—the *antherozoid*, and including (1) the ferns and their allies, the lycopods and equisetums, (2) the mosses and liverworts; and, thirdly, the Thallophyta, plants which form neither archegonium, ovule, nor pollen-grain, and including the two great divisions of Algæ and Fungi.

Of the two divisions of the Siphonogamæ, Angiospermæ and Gymnospermæ, the latter, in which, though fertilisation takes place by means of a pollen-tube, the egg is contained in a rudimentary archegonium, finds a most natural position at the end next the true Archegoniatæ. We may also note the tentative subdivision of the Angiosperms into Acrogamæ where the pollen-tube reaches the egg by means of the micropyle, and endosperm is formed after fertilisation, and the Chalazogamæ, where the mode of entrance is by way of the chalaza, while endosperm is formed before fertilisation. The former include everything except the Australian genus *Casuarina*, which constitutes the order Casuarinaceæ, and at present the subdivision Chalazogams. In the subdivision of the dicotyledons with free petals (Choripetalæ) there has been much alteration from Bentham and Hooker's separation into Thalamifloræ, Discifloræ, and Calycifloræ. Only the last of these remains as one of six, while the whole is enlarged

by the intercalation of the families formerly separated as the anomalous division Apetalæ. Some of these, like the Chenopodiaceæ and Amarantaceæ, fall naturally into their places, but the affinity of others, like Salicaceæ (the willows) is doubtful or quite unknown, while others again comprise distinct series. Under each natural order (the very rare or small ones are omitted) the most important characteristics are given, with the floral formula and the broad geographical distribution. In the case of larger orders, division into tribes is noticed, in the smaller only the endemic genera and the species of economic importance.

Professor Packard on Acquired Characters.

On the Inheritance of Acquired Characters in Animals with a Complete Metamorphosis. By Alpheus S. Packard, M.D. Reprint from the *Proceedings of the American Academy of Arts and Sciences*, 1894, pp. 331–370. Boston, Mass.

The historical *précis* with which Professor Packard opens his paper very naturally consists largely of an exposition and criticism of Weismannism. In this he makes one serious error. By quotations from the "germ-plasm," he supports his statement that "Weismann's reasons for not accepting the doctrine of transmission of acquired characters would appear to be purely hypothetical and *à priori*." It is certainly true that Weismann has adduced purely theoretical and *à priori* reasons. But again and again in published papers he has examined supposed cases of the transmission of acquired characters and of mutilations, and has shown the considerations of actual fact which support his hypothetical and *à priori* objections. As a matter of history, each term in the continuous adduction of fresh cases, or supposed cases, of such a transmission, has been brought forward after Professor Weismann and his supporters have shown reasons for not accepting preceding terms. The present position of the controversy is this. As yet no case of the transmission of acquired characters has been brought forward based on evidence accepted as cogent by Professor Weismann and his supporters. Professor Packard may say that this is so much the worse for the intelligence of Professor Weismann and his supporters. He may not say that their reasons are purely hypothetical and *à priori*.

Professor Packard reviews a number of the arguments brought forward by the advocates of this transmission and adds several of his own. We are, however, compelled to state that the arguments are not presented so as to convince any but the already convinced. Take one of his instances. He cites Paul Bert's experiments upon daphnids, in which he attempted to acclimatise these fresh-water crustacea to saline water by gradually adding salt. At the end of forty-five days, when the water of the aquarium contained 1·5 per cent. of salt, "all the Daphniæ had died; but the eggs contained in their brood-sacs had survived, and the new generation of Daphniæ, to which they had given birth, flourished perfectly well in the same medium." "We should interpret these facts," says the Professor, "as showing that the crustacean had been so profoundly affected in the lifetime of the individual as to produce young perfectly adapted to a changed environment. The germ-plasm may have been the vehicle, all the same, but the experiment is a case in favour of the neo-Lamarckian principle." Was ever argument less cogent? Here are animals supposed to transmit to their offspring a character before they have acquired it: to transmit to their eggs a capacity to endure salinity, to the non-acquisition of which they testify by their own deaths.

Moreover, the offspring, with respect to this character, are in a position in which it is impossible to judge of what, in their case, is inherited and what is acquired, for they are subjected to the new influence from their earliest birth. As in this instance, so in many others, Professor Packard walks round the point at issue. All instances of the power of environment to mould the individual are of the greatest value; and Professor Packard and others have done service in directing attention to a side of nature perhaps neglected. But, for this particular controversy, what is wanted is proof of the inheritance of the result of the moulding.

The actual occasion of this publication is a careful and valuable account of the complicated metamorphoses of many insects, accompanied by suggestions of causes in the environment which might have produced the characters of the stages in question. Students of evolution and of natural history will find this valuable, but we submit that none of it has a direct bearing upon the controversial problem placed as the title of the paper. In every case the argument goes no further than to suggest that, if acquired characters be transmitted, and if the Lamarckian factor in modification of organisms have the value assigned to it by Professor Packard, then the characters in these stages of larval history might be explained as due to the inheritance of acquired modifications. But all this, although it has a place, and a useful place, in scientific writing, takes us no further towards a solution of the problems in question, and gives support to the uneasy suggestion made at the recent discussion at the British Association that many of the opponents of Natural Selection do not understand the nature of the problems about which they write.

Zoology for Farmers.

Agricultural Zoology. By Dr. J. Ritzema Bos, Lecturer in the Royal Agricultural College Wageningen, Holland. With an Introduction by Miss E. A. Ormerod; Translated by J. R. Ainsworth Davis, B.A., Professor in the University of Wales. Pp. 256 and xx., with 149 illustrations. London: Chapman & Hall, 1894. Price 6s.

Dr. Ritzema Bos is so well known as a competent scientific naturalist who has specialised on what is called economic zoology, that we welcome an English rendering of his little book. It is designed to be a handy book for the practical farmer, and it deals with the animal kingdom in such a way that the private student shall have an intelligent idea of the general structure and organisation, and of the place in the scale of life of those animals that are harmful or helpful to agriculture. Reference to the domesticated farm animals is omitted, "for these are treated not by the zoologist but by the lecturer in stock-breeding." The translator, in his preface, says that he has made a few small additions and alterations, to adapt the book for those who live in England. Miss Ormerod, in her introduction, gives the book her blessing, and commends it to agriculturists.

The English is easy and intelligible. The illustrations, especially those dealing with insects and eelworms, are well adapted for their purpose. So far as the make-up of the book goes, we have praise for everything but for the lamentable absence of an index. The book ought to go into other editions. We hope that the technical instruction committees of County Councils will make large use of it for their teachers and for their students, but in another edition an index must be added—an index containing the common English names of all the animals referred to.

Dr. Ritzema Bos is a kindly man, and is ready to say a good word for wild animals as often as he can. Among mammals and birds he sets against each other the good and the harmful influences upon agriculture, and condemns unhesitatingly very seldom. Thus foxhunters will be pleased to learn that the fox "is perhaps generally of more use than otherwise to the farmer and the forester. It catches many rabbits, and also an enormous number of field voles, in the years when these become a pest. It also often eats insects (*e.g.*, cockchafers), worms, and snails." The stoat is decided to have a balance of good qualities, the weasel is almost uncompromisingly praised for its usefulness as a vermin destroyer; the common shrew, the lesser shrew, the hedgehog and the bats are all to be preserved. On the other hand, for the water shrew, the rats and mice, and the voles, Dr. Bos suggests a careful preservation of their natural enemies, and a bill of fare of which phosphorous paste, dressed in various attractive fashions, is the principal ingredient.

We are less satisfied with his treatment of birds. It is true that he advocates the preservation of all the British owls and of the kestrel and the buzzard. But he says that "predominantly harmful from killing domestic mammals" are the following species occurring in Britain: The sea eagle, the golden eagle, the peregrine falcon, the merlin, the hobby, the sparrow hawk, the goshawk, and the harriers. The result of the examination of stomachs proves that in many of these cases a large balance of good is done by the birds. As for the other cases, though it be true that *aquila non capit muscas*, the eagle destroys no insect pests, we have no sympathy for the naturalist who plays the informer. It is a poor consolation to know that few English farmers are likely to be driven by depredations on their flocks to set springes to catch golden eagles, or to go out with shot guns against sea eagles!

Among other birds the black list is less offensive to us, although we fear that the naturalist and the agriculturist will never quite see eye to eye in the matter of the destruction of any kind of birds. It includes the raven, the magpie, sparrows, and linnets. Because of its shortness it marks so great an advance on popular notions that we will pass it by in silence.

A good and much needed word is spoken for the persecuted toad. In addition to its attacks upon insects, the toad is a great destroyer of snails. "In the Research Garden attached to the Rouen Entomological Laboratory the snails were entirely exterminated in 1891 as a result of introducing 100 toads and 90 frogs."

Although we have occupied so much of the space at our disposal in discussing the part of Dr. Ritzema Bos' book that deals with Vertebrata, as a matter of fact the greater part of the book itself deals with injurious insects and worms. For all this we have nothing but praise. It would be of inestimable advantage to farmers and to gardeners were they to have an acquaintance with the facts about these common pests which are so clearly explained in this excellent treatise.

Monism.

The Monist. A Quarterly Magazine. Vol. iv., no. 4. Chicago: The Open Court Publishing Co. London: Watts & Co. Price 2s. 6d. July, 1894.

We fancy that monism, as yet, subtends a small angle in the minds of most of our readers; we may, therefore, take advantage of an article in the current issue of the *Monist*, written by the editor of the *Monist*, and

entitled "The Message of Monism to the World," in order to give a very brief account of the doctrine. Until a few years ago, in the minds of most people who reflected upon the meaning of things, and in the minds of each of the great mass of men who accepted with easy-going indifference what was thrust upon their consciousness, mental images of the universe were broken up among a series of separate identities. Thus, in the sphere of theology, there were the Creator and the things created; in psychology, there was the central ego, and the surrounding swirl of sensations and states of consciousness, there were mind and body acting and reacting upon each other. In science there was matter, and force acting on matter, while even to different kinds of matter and varieties of force there were ascribed separate existences.

In all these the attitude of the thinking world is slowly changing. Formerly, man attempted to express his sense of the dependence of all things upon a Supreme Being in the phrase "Omnipresence of the Deity"; now we attempt to express the same relations in the phrase "Immanence of the Deity." The change of phrase marks an attempt to destroy the sense of separation between the Deity and the universe, or, rather, to destroy the idea of the co-existence of two separate identities. Similarly, in psychology there is a tendency to be rid of a separate abstract metaphysical "ego," to merge the abstraction in the sum of states of consciousness. There is not an "I" which has sensations, emotions, and ideas; the sensations, emotions, and ideas are part of the "I." The change is better known in things scientific. When now we speak of matter and force acting and reacting upon each other, we are ready to admit that matter in the last analysis resolves itself into the action of forces, and that we know force only through its action on what we call matter. In the separate sciences the change of tendency is still plainer. We seek to interpret the elements in terms of each other, perhaps as unsuccessfully as Lord Salisbury seems to think, but with a hope greater than his, and we believe in the conservation of energy and in the transformation of chemical, electrical, physical, and perhaps even vital forces.

"Monism" is the pointed, progressive, and polemical expression of all this change of attitude. It is, to use the words of Dr. Paul Carus, "the new philosophy that is dawning upon mankind: the theory of oneness, which indicates that the world, we ourselves included, must be conceived as one great whole. All generalisations, such as matter, mind, and motion, are abstractions representing aspects of reality, but not entities or things-in-themselves, by a combination of which the universe has been pieced together. And all our notions of nature can be formulated in exact statements, which, when properly understood, form one harmonious system of natural laws." No doubt, like the active and aggressive form of every idea, it has coated itself with a certain brusque and repellent dogmatism; but soldiers actually on campaign are not beautiful objects: fine feathers, necessarily, are reserved for parade. But anyone who studies it may see that it is an honest attempt to combat the arrogant narrowness of agnostic and religious dogmatism.

Alaska.

Those interested in Alaska will find in the August number of the *Scottish Geographical Magazine* an epitome of the knowledge we possess of that little-known land, from the pen of Israel C. Russell. Mr.

Russell has made three journeys into the country himself, and he completes his account from the narratives of others who have travelled on various business in the country.

It is difficult at first to realise that Alaska is eighteen times the size of Scotland, and that it has a general coast-line of 4,000 miles, or, if the shores of the bays and islands be measured, of 11,000—12,000 miles. The river Yukon divides the country in two approximately equal portions. It rises in the north-western part of Canada, flows westward across the territory, and falls into the Behring Sea. The Yukon has a length of 2,000 miles, and drains an area equal to 440,000 square miles. It has every appearance of great age, and has been repeatedly disturbed and obstructed by volcanic action. The vast mountain system follows the coast, and is a continuation of the Cordilleras. The culminating peaks of the range are found in Alaska, and are Mount Logan, 19,500 feet, and Mount St. Elias, 18,010 feet. A host of other peaks, several of which exceed 14,000 feet in height, make this region one of the most rugged and impassable in the world.

The region is one of volcanoes, and eruptions of more or less magnitude are of frequent occurrence. Mr. Russell notes that Shishaldin, in the island of Unimak, is a symmetrical cone with gracefully curving sides of the same type as Fuji-San, and rises to the height of about 8,000 ft. The wreath of smoke proceeding from the crater is a well-known beacon to mariners. On the shores of Behring Sea and of the Arctic Ocean the lofty mountains disappear and give place to the "tundras" or low, nearly-level, moss-covered plains of 70—100 miles broad. During the summer these tundras are swampy and covered with mosses, lichens, small flowering plants, rushes, and ferns. The most conspicuous plants are the dwarf willows, which attain a height of about 2 ft. The soil is a black humus, and at a general depth of a foot or thereabouts is always frozen. This frozen subsoil is not confined to the tundras, but in many places along the Yukon river the banks are formed of horizontal sheets of ice many feet thick, covered with moss, and supporting a dense forest of spruce. In hot summer, when the temperature is from 90°—100° F. in the shade, one may brush away the moss at his feet and find solid ice beneath.

In some places a depth of 300 feet of black, peaty humus has been observed, and Mr. Russell notes the interest of these deposits in connection with the formation of coal.

The glaciers of Alaska are, with the exception of those of Greenland, the largest and most instructive in the Northern Hemisphere. The largest is the Malaspina, on the south of Mount St. Elias. Here numerous ice-rivers meet and unite in one great sheet about 1,500 square miles in extent. Mr. Russell states that the glaciers of Alaska are slowly retreating, and that this recession has probably been going on steadily for the past 100 or 150 years.

Mr. Russell points out that Alaska is not so densely forested as is generally assumed,—and that, in fact, the tree region is confined to the south-eastern part of the country, most of the western portion being treeless. The article concludes with some observations on the inhabitants, and will well repay perusal as a general summary of information on a region at present but little known.

We have received from M. Augusto Nobre the number for July, 1894, vol. i., no. 3, of that enterprising Portuguese publication, the *Annaes de Sciencias Naturaes*. Among other interesting matter it contains a

plate representing a bizarre-looking Selachian, captured at Buarcos. Mr. G. A. Boulenger, of the British Museum, who was consulted, refers it to *Squalus rostratus*, Marc, which Dr. Günther identifies with *Selache maxima*. Dr. Lopes Vieira, who gives an account of the specimen, is as yet by no means satisfied with the identification. From the figure and details given we are inclined to agree that if it is *Selache maxima* it is a very abnormal specimen.

In the last number of the *Journal of Anatomy and Physiology*, Mr. F. G. Parsons describes the tendo achillis of mammals, as being formed by a rope-like twisting of the fibres derived from the two heads of the gastrocnemius, the soleus, and the plantaris. The twisting which can be made out in man he regards as being partly due to the rotation of the limb in the embryo, but more largely to the position of the foot in the uterus of the mother.

Dr. A. Grandidier has issued two more volumes of plates illustrating the natural history of Madagascar. These are the fourth and fifth volumes of "Histoire Naturelle des Plantes," By H. Baillon. Once more we regret to observe that the plates are unaccompanied by any text or explanation.

In the *American Journal of Science* for August, 1894, Mr. O. L. Simmons, of Tufts College, Mass., writes on the "Development of the Lungs of Spiders." He comes to the conclusion that "the lung-book of the spider, and presumably of all arachnids which possess one, arises at first as an external structure upon the posterior surface of the abdominal appendages. These appendages sink in without any inversion or complications, in exactly the manner theoretically deemed probable by Kingsley, so that there can no longer be any doubt as to the exact homology existing between the lungs of the spider and the first pair of gills in Limulus."

The beautiful little fresh-water alga *Volvox* is well-known to lovers of ditch and pond life. We see from the July number of *The Botanical Gazette* that Mr. W. F. Shaw has found in a ditch at Palo Alto, California, an allied plant, which he considers to be and describes as a new genus, *Pleodorina*. The name indicates its affinity with *Eudorina*. It resembles *Volvox* more than does the latter genus in the number of cells composing the individual and the specialisation of certain cells for the purpose of reproduction. No sexual generation has, however, yet been observed.

OBITUARY.

GEORGE HUNTINGTON WILLIAMS.

BORN 1856. DIED JULY 12, 1894.

ONE of the most prominent members of the new school of Petrography in America has been prematurely removed in the person of Dr. G. H. Williams, Professor of Inorganic Geology in the Johns Hopkins University, Baltimore. Though only in the prime of life, he succumbed in July to an attack of typhoid fever; his health, it is supposed, having been enfeebled by his arduous duties last summer in the department of Mineralogy of the Columbian Exposition. Professor Williams was a native of Utica, N.Y., and graduated at Amherst College in 1878. He subsequently resided for a short time in Berlin, and then studied under Rosenbusch in the University of Heidelberg, where he obtained the degree of Ph.D. in 1882. The following year he became connected with Johns Hopkins University, and was Associate Professor there from 1885 to 1892, when he was appointed to the Chair he held at his death. The Professor was an attractive teacher, and numbered among his pupils many of the most successful workers in Petrography of the rising generation in America. Apart from his numerous original memoirs and the "Geology of Maryland," which we reviewed some months ago, his well-known "Text-book of Crystallography" will long keep his memory in the mind of students; and it is a distinct loss to science that his projected work on the microscopic structure of American crystalline rocks, in progress at the time of his death, should have been thus abruptly terminated.

THE death of M. DUTREIL DE RHINS has robbed France of one of her best geographers. M. de Rhins had reached Tibet in course of a journey from Turkestan to China and was murdered during a dispute over some horses. It is a satisfaction, though a poor one, to learn from the newspapers that the Chinese Government have promptly ordered the recovery of his papers and effects, and further suggested some reparation to his family. M. de Rhins was only forty-seven, but he has left a monument behind in his account of the geography of Central Asia.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

PROFESSOR DR. E. RATHAY has become Director of the "önologisch-pomologischen Lehranstalt" at Klosterneuburg, near Vienna, and Dr. C. Rechingen provisory assistant to the botanical chair at Vienna. Dr. René Koehler has been appointed Professor of Zoology to the Faculty of Sciences of Lyon, and Mr. Wm. B. Marshall, of the New York State Museum, Professor of Biology in Lafayette College, Easton, Pennsylvania. The Kansas State University has divided the subject of Geology between two chairs, giving stratigraphical and physical geology to Professor E. Haworth, and leaving palæontology in the hands of Professor Williston.

DR. WILLIAM KING, the late Director of the Indian Geological Survey, is on his way home to England. Mr. C. L. Griesbach has been temporarily placed in charge, and as he is the senior official, it is probable that he will succeed to the vacated post.

MR. PETER ESSLEMONT, the head of the Scottish Fishery Board, has just died. It is to be hoped that his successor will have a competent scientific acquaintance with the matters with which he has to deal. No more suitable successor could be found than Mr. John Murray, of the "Challenger." We understand that strong representations in his favour have already been made by a large number of scientific men, and we need do no more than endorse their recommendation.

DR. FELKIN has been appointed to represent the Royal Scottish Geographical Society at the meeting of the German Congress of Naturalists and Physicians, to be held in Vienna during the month of September.

DR. JULIUS WIESNER has been receiving the congratulations of his past and present pupils on the completion of the twenty-fifth year of his professorship, the twentieth year of his presidency over the Institute of Plant Physiology of the Vienna University, founded by himself, and, thirdly, on the occasion of his safe return from a long journey of exploration to Java. The Professor was presented with an address and a silver medal of great artistic value.

PROFESSOR DR. PFITZER, of Heidelberg, has been raised to the dignity of Geheim-hofrath. A committittee has been formed in Gera to erect a monument to the deceased ornithologist, Karl Theodor Liebe. The Zoological Garden of Berlin, having reached its jubilee, proposes to erect a monument to the energetic director, Dr. Heinrich Bodinus. Subscriptions may be sent to Mr. E. Weigel, 141 Frankfurterstrasse, Berlin, O.

A CURATOR is wanted for the Museum of the Institute of Jamaica, at Kingston, Jamaica. The appointment will be for three years, and is renewable at the end of that period. Applications should be addressed to the Secretary of the Institute of Jamaica, care of Messrs. H. Sotheran & Co., 140 Strand, London, W.C., not later than October 15, and should be marked "Curator, Institute of Jamaica." Further information may be obtained frcm Messrs. Sotheran. The salary offered is at the rate of £400 a year.

The Swiney Lectures on Geology, under the auspices of the trustees of the British Museum, will this year again be delivered by Professor H. Alleyne Nicholson, who will take for his subject "The Making of the Earth's Crust." The lectures, which are on Mondays, Wednesdays, and Fridays, from the 1st to the 26th of October, will be delivered in the Lecture Theatre of the South Kensington Museum, since the proposed and much-needed lecture theatre of the Natural History Museum is still *in nubibus*. We further understand that this is the last year, for the present, in which the post will be filled by the very popular lecturer, Dr. Nicholson. For the ensuing year, at least, the well-known anthropologist, Dr. J. G. Garson, has been appointed Swiney Lecturer, and is expected to deal with the Geological History of Man. Meanwhile the Natural History Museum at Torquay is to have its Lecture Theatre built out of the £1,360 subscribed as a memorial to the late Mr. W. Pengelly.

The new Norwich Castle Museum, of which we published a detailed description (vol. i., p. 692), is now nearly completed, and the removal of the collections is in active progress. The formal opening, by the Duke of York, is announced to take place next month.

Next month the local collections of the Natural History Society of Tromsö, Norway, will be removed from the present inconvenient house to a fine new stone building, which has been occupied during the summer by the Tromsö Centenary Exhibition. The Museum is of two storeys, with convenient work-rooms and an attic for stores, and has been built by the town and district, aided by a Government grant. The Tromsö Society, though small, displays remarkable vigour and has formed one of the finest collections of a local fauna and flora to be met with in any Continental Museum. It has also obtained a large series of fossils from Spitzbergen and Novaya Zemlya.

Mr. A. Haly, the Director of the Colombo Museum, Ceylon, has issued his report for 1893. The chief object of this museum is to give a full representation of the products of Ceylon, both natural and artificial, and Mr. Haly points out how necessary it is that such a collection should be accompanied by a complete collection of the literature relating to Ceylon. He does not, however, find much value in the "Descriptions of Ceylon species published in various scientific periodicals." "In most cases," he says, "the descriptions of insects are unaccompanied by any figures, and are quite useless for the purpose of naming the species described, apart from authoritatively named specimens. *Entomologists, please note!* This museum has recently acquired very fine specimens of the Dugong—male, female and young. The female is 11 feet long, and is said to be the largest ever seen. A new kingfisher has been found on the island, apparently allied to the S. Indian species, *Alcedo bavani* of Walden. A new starred tortoise has also been found, which appears to be peculiar to Ceylon. A fish of the genus *Duymaeria*, is the first representative of that genus recorded from the Indian Ocean. A process of preservation by gum and glycerine is now used for fish, crustaceans, starfish, and holothurians. Instead of cork for insect boards, Mr. Haly uses a mixture of sawdust and cocoa-nut fibre.

Mr. Zenker, head of the station at Taunde in the Cameroons, has sent a large collection of bird-skins to the Royal Zoological Collection in Berlin. Dr. Stuhlmann also has sent many interesting novelties from Usaramo, in East Africa. Both these collections will be described in the *Journal für Ornithologie*. *A propos*, we learn that Dr. A. Reichenow's work "Die Vögel Deutsch-Ostafrikas" will be published in the autumn of this year by Dietrich Reimer, in Berlin.

The Penzance Natural History and Antiquarian Society has issued its *Report and Transactions* for 1893–94. The most important paper is a critical list of the Lepidoptera of Cornwall and the Scilly Islands, by the President, Mr. W. E. Baily. Many local lists have already been published, but this is a more general summary

and includes the results of the author's own work during the last ten years. The Cornish Lepidoptera, as a rule, are larger than their congeners in other parts of Britain. Among brief notes, it is announced that a fine example of Risso's Grampus *(Grampus griseus)* was stranded near Marazion in April last, and that the skeleton is being prepared for description in the next part of the Society's *Transactions*. Details of the external characters of the animal are given in the present note. Meteorological observations used to be made at Penzance by Dr. Giddy, but no regular records were made between 1887 and 1893. The Corporation have now instituted systematic observations for the borough, and hope to bridge over this gap from private sources.

The Malton Field Naturalists' and Scientific Society has commenced the issue of a monthly record of observations on local Natural History, entitled *Naturalist Notes*. This is one of the most vigorous of provincial societies, and has already formed a museum in many respects unique; but we doubt the wisdom of its latest venture, which strikes us as an unfortunate substitute for a well-digested annual report. At the recent annual meeting Professor L. C. Miall retired from the Presidency, and was succeeded by Mr. A. S. Woodward, of the British Museum.

The Norfolk and Norwich Naturalists' Society has issued its transactions for 1893-94—vol. v., part 5. Mr. C. B. Plowright, M.D., has been appointed president for 1894-5. The retiring president, Mr. T. Southwell, F.Z.S., whose address appears in the present number of the *Transactions*, took for his subject the past and present history of the society, which is now in the twenty-fifth year of its existence. Many important and interesting contributions appear in the number, and there is issued with it a photograph of a specimen of the tropic bird *Phaëton æthereus* found dead at Cradley, Herefordshire.

The Midland Union of Natural History and Scientific Societies held its seventeenth annual meeting at Ellesmere, Salop, on Friday and Saturday, August 3 and 4. The proceedings commenced with the council meeting on Friday afternoon, and eleven delegates from the various societies in the Union attended. The report of the executive committee was received, and after considerable discussion adopted. It was decided among other items that papers competing for the Darwin Medal should be published within the area of the Union, and that the Union should be made primarily a record office for the Natural History of the Midlands.

The new executive committee consists of the secretaries or a representative of each society in the Union, and six elective members as follow: Professors Allen, Bridge, and Lapworth of Mason College, Messrs. Chase, Hughes, and Wilkinson, with Professor Hillhouse and T. V. Hodgson as secretaries.

The annual meeting was held later, and accepted the recommendations of the council; the other officers were unanimously elected, viz., Brownlow R. C. Tower as president, and E. de Hamel as treasurer. It was also decided to admit photographic societies into the Union.

A conversazione was held in the evening at St. Oswald's College, and attended by upwards of 200 members and friends. There was a fair number of exhibits, chiefly local, and the college museum was thrown open. One paper was read by Mr. Bellamy, of Oxford, on the Migration of Birds.

Saturday was taken up by two excursions, one for archæologists to Chirk Castle and Valle Crucis, Llangollen, conducted by Mr. A. T. Jebb; the other for biologists, round the meres and peat mosses of the neighbourhood, led by Messrs. H. J. E. Peake and J. A. S. Jennings. Both excursions were well attended and very successful, notwithstanding the heavy rain which came down in the afternoon.

The botanists succeeded in finding most of the local plants, including *Nuphar pumila*, *Lobelia Dortmanna*, *Alisma natans*, *Utricularia minor*, etc., etc. The meres were all found to be "breaking," Ellesmere and Whitemere with *Glœotrichia echinata*, Colemere and Crosemere with *Aphanizomenon flosaqua*; in

spite of this *Leptodora hyalina*, *Daphnia jardinii*, and *Bosmina coregoni* were taken. The two latter do not appear to have been recorded from this district before.

Several members of the Union prolonged their visit over the Bank Holiday, and were able to make a more exhaustive search in certain areas with good results.

The hospitality and generosity of the Ellesmere people were unlimited, and afforded striking evidence of the capacity of a small local society.

During the week ending August 11 the members of the Geologists' Association visited Shropshire on their long excursion, under the direction of Professor Lapworth and Mr. W. W. Watts. The headquarters during the whole visit were at Shrewsbury. On Sunday, July 29, many of the members visited the Breidden Hills. During the rest of the week the programme was as follows:—Monday, Visit to the Wrekin, under the direction of Dr. Callaway; Tuesday, Visit to the Longmynd with Professor Blake; Wednesday, the eastern portion of the Shelve area, under the direction of Messrs. Lapworth and Watts, who were also the leaders on the following day, when it was arranged to visit the more westerly parts of the same district and to see the laccolite of the Corndon. Unfortunately heavy rain delayed the start, so that the former part of the programme had to be omitted. On Friday the famous Onny Section was examined, under the guidance of Professor Lapworth and Rev. J. D. La Touche. On Saturday morning the Mayor of Shrewsbury received the members at the Museum, which is of considerable interest. Later in the day Buildwas Abbey and the Severn Gorge at Coalbrookdale were visited, and the excursion brought to a close.

A valuable pamphlet on the "Geology of Southern Shropshire," by Lapworth and Watts, was issued to the members, and will shortly be published in the *Proceedings* of the Association.

While we are finding coal and ironstone at Dover, and while Professor Boyd-Dawkins is advocating a search for coal in Oxfordshire, arguments similar to those employed by English geologists for England have led the geologists of New South Wales to the discovery of coal under Cremorne in Sydney Harbour. Of late years the existence of coal under Sydney has been regarded by local geologists as almost a certainty, the only divergence of opinion being about the depth at which it was likely to occur. A seam of coal has now been found by boring in the above locality, at a depth of 2,801 and 2,917 feet, which varies in thickness from 7 feet 3½ inches to 10 feet 3 inches, and contains only minute bands of shale. The coal is good and especially suitable for steaming purposes. An account of the various borings is given by Messrs. T. W. E. David and E. F. Pittman in the *Records of the Geological Survey of New South Wales*, vol. iv., pp. 1–7.

At a meeting of the Linnean Society of New South Wales on June 27, Mr. Hedley exhibited the shelly tubes of *Kuphus arenarius*, collected by Mr. Brazier on a coral reef at Florida Island, Solomon Islands. As the affinities of this animal are somewhat problematical, visitors to the Solomons should endeavour to procure it.

Prince Henri d'Orleans arrived at Tamatave some weeks ago, and proceeded north to the place where Mr. Muller, the French explorer in Madagascar, met his fate a few months ago.

The library of Professor Carl Vogt, of Geneva, has been purchased by the Government of Roumania.

Mr. Frank Finn, M.A., F.Z.S., has been appointed First Assistant-Curator in the Zoological Department of the Indian Museum at Calcutta.

Dr. Henri Filhol has been nominated to the chair of Comparative Anatomy at the Natural History Museum, Paris, lately vacated by the death of Mr. Georges Pouchet.

CORRESPONDENCE.

The Art of Cataloguing.

Sir,—Whoever reviewed Mr. Seward's Catalogue of the Mesozoic Plants in the British Museum in your August number (p. 147) is no doubt a competent botanist, but can know very little about museum work. In cataloguing a series of fossils, whether plants or animals, "a bare quotation of the register numbers," would by no means serve the purpose, though your reviewer seems to think it would. In the first place, register numbers have an unfortunate habit of coming off, and it is exceedingly useful to have some subsidiary means of identifying the specimens. Secondly, a register number alone conveys no information to a worker at a distance; and it is surely advisable that such an one should be informed whether a specimen is a mere fragment, or shows some structural features, so that when he comes to the Museum he may be able to say at once exactly which specimens he wishes to see. At the worst, the system adopted by Mr. Seward, as by some other cataloguers before him, can do nothing more terrible than take up the space of which your reviewer seems so jealous; and I venture to submit that the space is by no means wasted.

Geological Department, British Museum. F. A. Bather.

Nascent Elements.

With regard to the question of nascent elements, has it been considered whether the conditions which apply to several gases in the above state also apply to them when solid, *i.e.*, if hydrogen could be evolved under conditions consistent with a solid state, what peculiarities different to solid hydrogen, evolved and then solidified, would it exhibit? Also when, say, carbon is freed from a compound does *it* exhibit any nascent phenomena?

Would not a solution of this problem shed light on the supposed action of chlorophyll in the formation of starch from CO_2 and H_2O in plants?

Hoping you will discuss this question in your valuable magazine.

Jno. E. Hardman.

78 Chorley Old Road, Bolton. E. P. Greenhalgh.

[Whatever may be the conditions requisite for the evolution of solid hydrogen, those existing in the leaves of living green plants must be so vastly different as to render comparison useless. As regards carbon, it is extremely improbable that the free element enters at all into the process of formation of organic matter from carbon di-oxide and water; it is generally supposed that the CO_2 loses only half of its oxygen, the remaining group (CO) combining with the hydrogen of the water to form a compound containing the three elements in these proportions (CH_2O). This compound is a much simpler one than starch, which is only deposited by the protoplasm when the amount of carbohydrate material present in the cell is in excess of that needed for the immediate use of the plant.—Editor.]

Natural Selection.

In answer to Professor Henslow and "Entomophilos," I may perhaps state that the various facts on which my statements were based are contained in my own and other papers, to which I must refer them. The word "apparently" in the phrase they object to might be replaced perhaps by "possibly" or "probably."

Natural Selection, like Inheritance of Acquired Characters, is a convenient working hypothesis, and has still to be *dis*proved.

The first part of Entomophilos's letter I do not follow. By the statement I made I did not intend to imply more than a resemblance between our flora and those of Northern Europe. It has been abundantly shown that the circle of visitors to a given flower is of a very definite nature, and from the fact that in our fauna there is a "natural preponderance of flies over other insects," it does not follow that there will be a similar preponderance of flies among the insect-visitors of certain flowers.

John C. Willis.

Discussions at the British Association.

I was present at the discussion on Natural Selection in the Subsection of Zoology, and feel strongly that the method adopted was not that most calculated to lead to useful result. After a number of interesting objections to Natural Selection had been urged, the most notable defenders of Darwinism contented themselves with implying either that their adversaries did not understand what they were talking about, or that they were unacquainted with the writings of Darwin. I wish to support the judicial proposal of Sir Edward Fry, that these discussions should be conducted like a case in a court of law, and that the counsel on either side should be kept to the point at issue. I venture to suggest that, instead of lumping together a somewhat heterogeneous set of papers, and then taking a discussion on the lot, it would be well for the secretaries of the organising committees to settle beforehand on definite subjects, or, better still, on definite propositions, and then to get specialists to contribute arguments from their own knowledge on either side of the case

Gallio.

Corrigendum.

C. H. B. Woodd, of Roslyn, Hampstead, calls our attention to an unfortunate slip in the July number. Natural Science, vol. v., p. 16, 6th line from bottom of page; for "were interchangeable terms," read "were not interchangeable terms."

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, 5 John Street, Bedford Row, London, W.C.

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NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 32. Vol. V. OCTOBER, 1894.

NOTES AND COMMENTS.

Protozoa from pre-Cambrian Rocks.

ONE of the most interesting contributions to geological and biological science of late is Monsieur L. Cayeux's report on the supposed Radiolaria and Foraminifera from the pre-Cambrian rocks of Brittany. About two years ago we alluded to Dr. Charles Barrois' discovery of these remarkable fossils, and announced the preparation of a detailed report by M. Cayeux. The results have now appeared in the *Bull. Soc. Géol. France* (ser. 3, vol. xxii., pp. 197–228, pl. xi.), and in the *Comptes Rendus* (vol. cxviii., pp. 1433–1435); and there is a valuable, cautiously-worded notice by Dr. George J. Hinde in the last number of the *Geological Magazine* (Dec. 4, vol. i., pp. 417–419). It appears that the supposed organisms occur in thin layers of flinty rock interstratified with the pre-Cambrian schists, and they are irregularly distributed, sometimes being met with singly, sometimes grouped together in clusters. They are much more minute than any known Radiolaria, the average size of those of the Palæozoic rocks being about seventeen times as great. The majority are simply spherical in form, some are ellipsoidal, and a few are inflated or bell-shaped. Some specimens exhibit radial spines, while others display an inner concentric shell connected by rays with the outer test; and nearly all of the best preserved examples have a lattice-like structure. M. Cayeaux recognises forty-five different forms of these fossils, and assigns the majority of them to known genera, while the whole may be comprised in the subordinal divisions of Radiolaria established by Haeckel.

Many of the minute bodies found with the Radiolaria, and originally supposed to be of the same nature, are now regarded by M. Cayeux as Foraminifera; and these also, it is interesting to note, differ from later forms in being much more minute. Some of the shells are

simple, others compound, and the latter consist of from two to seven spherical or ovoid chambers of different sizes aggregated together. The walls are very finely perforate, and some of the chambers are provided with one or more short blunted spines.

Much discussion will doubtless arise in reference to these new problematical bodies, and, so far as we can judge, there is some hope that they will stand the test of criticism better than the ill-fated *Eozoon*. Much confirmation, however, is yet required, and we look forward with interest to the discovery of similar forms in the pre-Cambrian rocks of our own island.

Primæval Life.

Theories as to what the nature of the earliest life must have been are still flourishing, and in the August number of the *Journal of Geology* Professor W. K. Brooks has a fascinating, interesting, and exceedingly suggestive, if not absolutely convincing, article on the subject. The paper bears so directly on the discussions at the recent meeting of the British Association as to the distribution of marine animals, that we give here an outline of its main points, which we hope will send our readers to the original. In many respects it reminds us of the views of the late Professor Moseley.

The variety, distinctness, and advanced structure of the animals found in the very lowest fossiliferous rocks have long puzzled geologists, and have seemed, on the theory of evolution, to necessitate an enormous backward extension of life before the beginning of the Cambrian period, although no tangible proof of the existence of such life is forthcoming in rocks where it ought to be found. It is this difficulty that Professor Brooks attempts to explain.

In a few vivid pages we are shown how the ocean is almost destitute of plant-life, how its flower gardens are stocked only with animals, and how its very herbs and lichens are corallines and sponges; we learn how the vast animal armies of the sea attack and devour other animal armies, but have never a plant for their forage; and thus, by descending steps, we are brought to the conclusion that "the basis of all the life in the modern ocean is found in the micro-organisms of the surface," lowly animals and plants so abundant and prolific that they meet all demands. These minute pelagic creatures must have been the first to exist, and where they first appeared there they have ever since remained, undisturbed by varying environment or the stress of competition, and therefore retaining their primæval simplicity.

The early pelagic fauna gave rise to a few simple types, such as we now know only in the larval forms of higher animals. But further development never took place here.

On the contrary, we recognise that all highly organised marine animals are products of the bottom or of the shore, or, as in the case

of the whales, of the land; even the pelagic animals of higher grade have come back again to the surface from ancestral homes elsewhere.

There was, however, a time when the bottom and shores of the ocean were untenanted; when, perhaps, physical conditions made their population impossible. Life at that time occurred only in simple forms and near the surface of the central sea. At last the bottom was discovered and colonised. The earliest settlements were not in shallow water, where food supply was scanty and mixed with sediment, nor in the great depths as yet unfavourable to life; but it is inherently probable, as well as confirmed by palæontological evidence, that the bottom life first found a footing in the deep water around elevated areas. Colony after colony may, it is true, have been "swept away by geological change like a cloud before the wind"; but when once the outlines of our modern continents were blocked in, then "the first fauna which became established in the deep zone around" them "may have persisted and given rise to the modern animals."

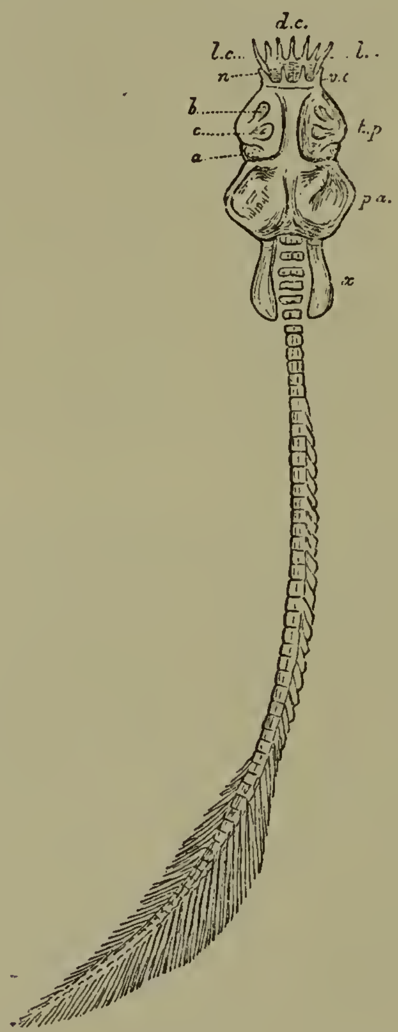
The sudden and enormous change of conditions accompanying this change of residence soon made itself felt on the creatures of the deep. The results were increase of asexual multiplication and the establishment of colonies, crowding followed by competition and more exacting selection, the acquisition of hard parts, and increase of size. Progressive evolution along these lines would have required no lengthy period to develop, out of those pelagic and soft-bodied ancestors that geologists will never discover, a fauna in all respects similar to that which geologists have already discovered in the Lower Cambrian rocks.

Thus those æons which the geologist has loved to imagine as his own, which the evolutionist has demanded, but which Lord Kelvin has denied and Lord Salisbury ridiculed,—they for Professor Brooks are but the baseless fabric of a dream, unproved, unreal, and unneeded.

The Primæval Lampreys Again.

Even of the earliest animals which possessed well-developed hard parts, we still know very little; and it is strange how peculiarly accidental are the few important discoveries that have been made. The little Devonian lamprey *Palæospondylus*, for instance, is known to us merely through the circumstance that the Caithness quarrymen happened to alight upon a little patch of good flagstone at a remote spot named Achanarras. It has been found hitherto only in that one quarry and is restricted to one narrow layer of rock. *Palæospondylus*, however, revolutionises our old ideas of the Class Marsipobranchii, and we have already published communications in Natural Science pointing out its significance. Quite lately still more information has been obtained on the subject, and Dr. Traquair's latest restoration of

the skeleton, taken from the newly-issued part of the *Proc. Roy. Phys. Soc. Edinburgh* (vol. xii., pp. 312–320, pl. ix.), is reproduced below for comparison with those previously attempted. It now appears that the animal had a median opening or ring, surrounded with feelers, at the front of the head; this being presumably comparable with the nose of a modern lamprey. It is also evident that the fin-supports at the end of the tail are longer than previously suspected; and those at



Palæospondylus gunni; new restoration by Dr. R. H. Traquair, somewhat enlarged. *d.c.*, dorsal cirri; *l.c.*, lateral cirri; *n.*, nasal opening; *p.a.*, periotic region of skull; *t.p.*, anterior part of skull, with indents marked *a.*, *b.*, and *c.*; *v.c.*, ventral cirri; *x.*, problematical (? branchial) plates.

least of the dorsal aspect are proved to bifurcate towards the top, exactly as in a lamprey. That *Palæospondylus* is a primæval Marsipobranch is thus suggested still more forcibly than ever, and the next important step is to discover the links between this little organism and some of its yet more remarkable contemporaries, *Pteraspis* and *Cephalaspis*.

The Armoured Fore-runners of the Chordata.

Remarkable additions to our knowledge of the last-named animals, now commonly assigned to a subclass termed Ostracodermi or Ostracophori, have lately been made by Dr. J. V. Rohon and Dr. Friedrich Schmidt in the publications of the St. Petersburg Imperial Academy of Sciences. The descriptions, both of the form of the shields of *Tremataspis*, *Auchenaspis* (*Thyestes*), and fragments of allied genera, are supplemented by a detailed, well-illustrated account of

their minute structure. Moreover, the explorations of Herr Simonson in the Upper Silurian Limestone of the Island of Oesel, in the Baltic, have recently furnished most of the principal European museums with good specimens. The fossils are thus within reach of most workers. The greatest sensation of late, in connection with this subject, however, is the announcement by Mr. William Patten, in the *Anatomischer Anzeiger*, that certain parts of the shield of the existing king-crab, *Limulus*, are almost identical in intimate structure with a certain layer in the shield of the Devonian *Pteraspis*. We have not yet had the opportunity of verifying the observation; but if the facts can be established Palæontology will, after all, furnish some support to the idea that there is a close connection between the primitive Chordata and the so-called Arachnids of the *Limulus* tribe. We prefer, however, at present to leave the question of the ancestry of the Chordata to the Embryologists, and we cannot do better than allude next to recent researches in this direction.

The Ancestry of the Chordata.

The researches of the past ten years have shown a marked tendency towards a closer and closer *rapprochement* between those three groups of enterocœlous animals, the Echinoderma, the Enteropneusta, and the Chordata. The idea of an affinity between the two first groups dates from the time when Agassiz in America and Metschnikoff in Europe discovered that *Tornaria*, which had been regarded by Johannes Müller as a starfish larva, was in reality the larva of the worm-like *Balanoglossus*. Metschnikoff even went so far as to homologise the proboscis of the adult *Balanoglossus* with an ambulacral tentacle, and the proboscis-vesicle he regarded not simply as a rudimentary, but as an actually vestigial representative of the water-vascular system of the Echinoderm. It is chiefly to Mr. W. Bateson, on the other hand, that we owe the establishment of the Chordate affinities of the same remarkable animal; while the whole progress of Echinoderm research, in the able hands of Ludwig, Semon, Bury, and others, has revealed a continually-increasing series of fundamental resemblances between the Echinoderm and the Chordate types. The idea of a real affinity between the three groups has taken deep root, especially in England, and the somewhat ill-founded opposition of Spengel to the view of the Chordate relations of *Balanoglossus* has effected little beyond the strengthening of the position of those who dissent from his conclusions.

Mr. Garstang expounded a theory of the inter-relationships of these groups, which he takes to present a simple and consistent explanation of the structure and development of *Balanoglossus* and the lower Chordata.

Recent research has shown that the larvæ of Echinoderms are all derivable from one common type, which can be imagined as a

simple *Auricularia* larva, possessing a terminal blastopore, a continuous circumoral (longitudinal) ciliated ridge, with a subjacent central nervous system, and an adoral ciliated band, which extends round the anterior and lateral margins of the mouth, and is drawn inwards and backwards in the mid-ventral line to form a ventral œsophageal ciliated loop.

The *Tornaria*-larva of *Balanoglossus* is equally derivable from the *Auricularia*-type; and now Mr. Garstang suggests that the true Chordata also trace their origin from the same primitive pelagic form, thus following out in a definite manner the interesting views of Brooks in this direction. (See NATURAL SCIENCE, vol. iii., p. 222.)

The basis of his theory is the homology which he proposes between the medullary folds of vertebrate embryos and the paired circumoral longitudinal ridge of *Auricularia*, the homology being founded on the identity of the relations of these two sets of structures to the mouth, the apical pole, the blastopore, and the central nervous system. In the Chordata the ciliated medullary ridges eventually fuse to form the medullary or neural canal, thus enclosing the sense-organs of the apical pole (optic pits) and the blastopore.

In the Echinoderma, as was shown by the beautiful researches of Semon, the central nervous system becomes segregated at an early period from the ciliated ridges, and migrates ventrally to form the circumœsophageal nerve-ring, while the ridges eventually atrophy and the blastopore persists as the anus.

An especial importance naturally attaches to the relations of the nervous system and ciliated bands of the developing *Balanoglossus*. The diagrams exhibited at the reading of the paper, and the new point of view from which the author regarded the course of the nerve-tracts in *Balanoglossus*, undoubtedly revealed some most significant, if not decisive, evidence in favour of his theory. Unlike his predecessors, Mr. Garstang holds that the dorsal nerve-cord in the trunk of *Balanoglossus* is not the morphological continuation of the collar-cord, but is —along with the ventral cord—a nerve-tract that is entirely confined to the group Enteropneusta, developed *pari passu* with the posterior elongation of the body. He holds, on the other hand, that the collar-cord, though ontogenetically median and unpaired, has been phylogenetically derived by the fusion of two lateral cords, and he finds support for this view in its divarication posteriorly to form the nerve-ring which runs along the posterior rim of the collar. The collar-ring is upon this view homologous with the periblastoporal portion of the nerve-plate in vertebrate embryos, and with the posterior transverse loop of the central nervous system of *Auricularia*.

As to the medullary folds of the collar which give rise to the neural canal, it was pointed out that, according to the researches of Morgan on *Tornaria*, the medullary folds do not cease at the posterior limit of the collar-cord, but bend round the sides of the body and *persist through life as the posterior rim of the collar*, the area of separation

in the mid-dorsal line being reduced and obliterated by the union of the longitudinal portions of the folds to form the neural canal. Attention was also drawn to the facts that the medullary folds of *Tornaria* not only arise in close relation with the degenerating posterior portion of the ciliated band, but take a course which is identical with it; and that the region of the neural tube is just that area where the convoluted ciliated bands approximate most closely in the dorsal region of the body.

The position of the gill-slits in *Balanoglossus* behind, and in Chordata in front of, the neural loop was attributed to that peculiar change of position which the primitively cæcal gill-pouches of *Balanoglossus* undergo during the period of metamorphosis, as observed and described both by Agassiz and Morgan.

Pending the appearance of the author's complete account, we need say no more here, except, perhaps, that Mr. Garstang finds in the peculiar adoral ciliated band of *Auricularia* the exact precursor of the combined peripharyngeal bands and the marginal bands of the endostyle of the Tunicata and *Amphioxus*.

The Ancestry of Modern Mammalia.

In the *American Journal of Science*, Professor O. C. Marsh is continuing his valuable brief notes on the remains of Tertiary mammals from the western regions of North America, while Professors Cope, Scott, and Osborn, and Mr. Earle, are engaged upon more elaborate memoirs. Progress is thus rapid in the northern half of the New World, and Mr. Lydekker has spent the autumn in further work upon the remarkable extinct Tertiary mammalia, chiefly sloths and armadillos, in its southern half. Of greatest interest, however, is the announcement that the general public in North America will soon have the opportunity of seeing these extinct quadrupeds displayed in an easily comprehensible manner, so that they can judge of the beasts for themselves. Except at Princeton, the collections of mammalian remains have hitherto been stowed away almost like goods in a store; but the American Museum of Natural History has now for some time secured the services of several good collectors, including Mr. J. B. Hatcher, and numerous entire skeletons of the most typical Tertiary animals are, as a result, at present being mounted for the exhibition rooms. Professor Osborn, of Columbia College, is superintending the work, and as the mounting proceeds, drawings of the actual specimens will be published.

European Pleistocene Mammalia.

In Europe similar studies are also progressing favourably, and an enterprising dealer, we understand, has been making extensive excava-

tions in the Pliocene deposits of Pikermi and the Island of Samos. Dr. Forsyth Major's memoir on the Pliocene Mammalia of Samos will soon appear in the *Palæontographica*, and his collection from Olivola, in the North of Italy, is now being extricated from matrix at the British Museum.

Much greater progress, however, has been made of late in the study of Pleistocene Mammalia. M. Edouard Harlé has been especially energetic in the South of France, and has found much of interest in the caverns of Haute Garonne (*Bull. Soc. Géol. France*, ser. 3, vol. xxii., pp. 234–241, and *L'Anthropologie*, vol. v., pp. 402–406). His most interesting discoveries consist in further evidence of the occurrence of the striped hyæna, and of the elk in Southern France. Professor Lartet, thirty years ago, was of opinion that the striped hyæna migrated northwards from Africa beyond the Pyrenees, at the time when Europe and Africa were connected by barriers; but considering that teeth almost identical with those of the striped hyæna have also been found in the Pliocene both of France and England, M. Harlé now regards it as much more likely that the animal met with in the caves was a direct descendant of its predecessor in the European region. The confirmation of the discovery of the elk in south-west France is interesting, because the evidence has not previously been very satisfactorily described. Mr. Harlé now gives a figure and description of an upper molar tooth, which he considers to be unmistakable.

Pleistocene Mammalia from the Thames.

In England, one of the most interesting discoveries of Pleistocene bones lately made is that described by Messrs. J. R. Leeson and G. B. Laffan in the last number of the *Quart. Journ. Geol. Soc.* These authors have found a bed of loam deep in the Thames gravels at Twickenham, and here, or immediately above it, there occur the remains of the reindeer, bison, and oxen. The only known fragment of the Saiga Antelope found in Britain was also obtained from the same neighbourhood. One reindeer antler and a skull of bison are especially fine, and these specimens have been presented by Dr. Leeson to the British Museum. The most interesting feature of the discovery, however, is the possible occurrence of the Celtic shorthorn in association with the above-mentioned remains. We are by no means convinced that the authors have made out their case for this modification of current views; for all discoveries hitherto have tended to show that the shorthorn was introduced into Britain by the Neolithic people who invaded the country after it became an island. Nevertheless, the mode of occurrence of the bones at Twickenham is suggestive of their all belonging to one period, and there seems to be no doubt that some specimens do belong to the small ox in question.

Acclimatised Rats and Cats.

A new interesting case of the gradual adaptation of animals to their surroundings is quoted in a recent number of *Public Opinion* from the *Pittsburg Dispatch.* It appears that in the cold-storage warehouses in Pittsburg there were originally no rats or mice. The temperature in the cold rooms was too low. The keepers soon found, however, that the rat is an animal of remarkable adaptability. After some of these houses had been in operation for a few months, the attendants found that rats were at work in the rooms where the temperature was constantly kept below the freezing point. They were found to be clothed in wonderfully long and thick fur, even their tapering snake-like tails being covered by a thick growth of hair. Rats whose coats have adapted themselves to the conditions under which they live have domesticated in all the storage warehouses in Pittsburg. The prevalence of rats in these places led to the introduction of cats. Now, it is well-known that pussy is a lover of warmth and comfort. Cats, too, have a great adaptability to conditions. When cats were turned loose in the cold rooms they pined and died because of the excessive cold. One cat was finally introduced into the rooms of the Pennsylvania Storage Company which was able to withstand the low temperature. She was a cat of unusually thick fur, and she thrived and grew fat in quarters where the temperature was below 30 deg. By careful nursing a brood of seven kittens was developed in the warehouse into sturdy, thick-furred cats that love an Icelandic clime. They have been distributed among the other cold storage houses of Pittsburg, and have created a peculiar breed of cats, adapted to the conditions under which they must exist to find their prey. These cats are short-tailed, chubby pussies, with hair as thick and full of under fur as the wild cats of the Canadian woods. One of the remarkable things about them is the development of their "feelers." These long stiff hairs that protrude from a cat's nose and eyebrows are, in the ordinary domestic feline, about three inches long. In the cats cultivated in the cold warehouses the feelers grow to a length of five and six inches. This is probably because the light is dim in these places, and all movements must be the result of the feeling sense. The storage people say that if one of these furry cats is taken into the open air, particularly during the hot season, it will die in a few honrs. It cannot endure a high temperature, and an introduction to a stove would send it into fits.

Living Specimens of Protopterus.

Those zoologists who attended the conversazione given by the local committee to the members of the British Association in the University Museum, had the opportunity of seeing a zoological exhibit of very unusual interest. Dr. H. O. Forbes, the Director of Museums, Liverpool, exhibited on behalf of the Derby Museum living specimens

of the African dipnoid *Protopterus annectens*, encased in the blocks of mud in which, during the dry season, these singular fishes bury themselves. They had been obtained for the museum from one of the West African rivers, at a distance of nearly 500 miles from the sea, through the exertions of Mr. Ridyard, of Liverpool. The fish had been in their mud-cocoons for over five months, having been kept in England for more than two months. Dr. Forbes prepared in the large lecture room of the University Museum glass jars of tepid water and into these the cocoons were placed in the presence of visitors. The hard rock-like clay became softened, and one of the fish soon came out in a lively condition and swam about freely. Mr. Forbes stated that while some specimens in his possession had emerged in five minutes, others had taken at least an hour. Of course, living specimens have been brought to Europe in this condition before now —there are at present a number of specimens in the reptile-house of the Zoological Gardens—but we fancy that very few of the members of the Association have had before, or are likely to have again, so interesting a practical demonstration of the habits of these animals.

The Plague Bacillus.

An authoritative account of the bacillus of the Oriental plague is at length to hand from the pen of its discoverer, Professor S. Kitasato of Tokio. We learn from the *Lancet* of August 25 that it is a capsuled bacillus, scarcely, if at all, motile, and exhibiting a tendency to bipolar staining. It is present abundantly in the blood and internal organs, and especially in the spleen and bubonic swellings of victims of the disease. It can be grown readily on suitable cultural media at the temperature of the human body, and rodents inoculated with pure cultures die in from two to five days, with symptoms and pathological changes resembling those of the natural disease in man. Rodents seem, indeed, to be highly susceptible, rats and mice having died in large numbers in Hong Kong during the epidemic, as they are related to have done during the historic visitations of the disease in this country.

All these facts are in complete harmony with what might have been anticipated, and the excellent work done by Professor Kitasato in Koch's laboratory in Berlin should leave little room for doubt that his discovery is a genuine and an important one. As was natural, however, the supremacy of this particular organism has not been allowed to pass unchallenged, and other claimants for the doubtful honour of being the real cause of the plague have been put forward by Dr. Yersin and others. Final judgment as to the respective merits of the rival microbes must be postponed till fuller details are forthcoming. No doubt securely bottled samples will in time arrive in this country, where it is to be trusted they will be kept under lock and key. Professor Kitasato has, however, already shown that the powers

of resistance of his plague bacillus are not very high, as it seems to form no spores. Four days desiccation kills it, as does exposure to sunlight for three or four hours in the dried condition. The temperature of boiling water is fatal in a few minutes, and a one per cent. solution of carbolic acid kills in an hour. So that we shall know in a measure how to deal with this bacillus in case it ever does break loose among us. It seems, moreover, that the plague is essentially a filth disease, and would stand little chance in presence of decent modern sanitation.

Small-pox.

The recent outbreak of small-pox in London, though the prompt measures taken seem to have been effectual in confining it within moderate limits, may serve to remind us how difficult a thing it is to fight against an enemy whose exact nature we do not know. Empirically we have learned much as to the prophylaxis and treatment of the disease, and now that experiment has demonstrated that vaccinia is merely small-pox modified by transmission through the cow, we have a rational scientific basis for the practice of vaccination. Yet, though small-pox is always with us, and in spite of the fact that experimental inoculation of mankind with its attenuated virus is rendered compulsory by law, the actual nature of the virus remains still under dispute. It is rather the fashion nowadays, when no bacterium can be demonstrated in a given specific disease, to invoke the Protozoön. That Protozoa may cause disease we know—witness malaria—but the utmost caution is required in interpreting the appearances to be seen in epithelial cells stained with fancy dyes, and the protozoan origin of small-pox, as of cancer, rests at present on a very slender foundation. And indeed there is now some evidence that the virus of small-pox may after all be a spore-bearing bacillus which has hitherto eluded observation by its habit of turning into an unstainable spore at an earlier period than that at which anyone had thought of looking for it. Both Dr. Klein and subsequently Dr. S. Monckton Copeman have independently discovered that such a spore-bearing bacillus is to be demonstrated in the lesions of small-pox and vaccinia, if search be made at a sufficiently early stage of the disease, and though cultivations have uniformly failed, there seems at least to be a possibility that this organism may prove to be the true cause of small-pox.

In a supplement dated 25th May last the *Leeward Islands Gazette* reports a paper read by Mr. C. A. Barber at the inaugural meeting of the St. Kitts branch of the Leeward Islands Agricultural Society. The paper deals chiefly with a disease affecting the sugar-cane, known as the Rind-fungus (*Trichosphœria Sacchari*), and described as the most

dangerous and widespread enemy of the cane cultivation of the present day, occurring in greater or less abundance probably everywhere where the plant is grown. Associated with the fungus is a moth-borer caterpillar; in fact, the insect seems to be the first cause of the disease, as the fungus cannot penetrate the sound plant. The young grubs immediately after hatching upon the leaf-blades make their way down to the protecting leaf-sheath, where they burrow in the soft inner tissues till strong enough to penetrate the cane. The burrows of the moth-borer give entrance to the fungus, and are noticeable as centres of infection. The fungus then rots the cane Various suggestions are made for the destruction of the moth-borer, and the selection of canes capable of resisting the attacks of the fungus.

LET the humble student of British botany rejoice. A month or two ago we referred to the Brambles as the happy hunting grounds of a little band of clerics. But the Hawkweeds seem even more remunerative, for in the *Journal of Botany* for August, Mr. F. J. Hanbury, who is tardily and expensively monographing the genus, diagnoses no less than seven new species and two-and-twenty new varieties. As their names indicate, two, *Hieracium surrejanum* and *H. cantianum*, grow close at home, in fact, within fifty miles of London. We do not dread their extermination, in spite of such proximity, except by accident, for like the Brambles and Roses these species are beyond the ken of the ordinary collector, and to quote Mr. Hanbury himself in his mild censure of his Scandinavian colleagues, we "feel it necessary to express a grave doubt as to the practical utility of of such subdivision," and our fear that the study of these genera will be rendered an impossibility, except to the few specialists who may devote their entire lives to their elucidation.

A NOTE on the germination of seeds in sawdust in the *Botanical Gazette* for August will be of interest to teachers of Botany. When all goes well there is no better way of getting clean straight roots for class or experimental purposes; but, unfortunately, the sawdust has frequently a detrimental effect, and roots of plants germinated therein are seen to be in a very unhealthy condition. This, Mr. Stone points out, is due to the presence of tannin, and if only care be taken to select dust free from this, results will be quite satisfactory. Roots grown in sawdust containing tannin show a reddish colouration, are crooked, and much reduced in size, and this abnormal appearance may be brought about by saturating any sawdust which has been proved suitable for normal growth with a one per cent. solution of tannin. Oak and chestnut dust are, for this reason, to be avoided, while that obtained from Conifers has no prejudicial effect. The writer adds: "We have never experienced difficulty with any sawdust which failed to give the tannin reaction, and during the past year we have used the same sawdust continually without changing."

In the same journal is a *résumé*, by J. C. Bay, of observations and experiments in the formation of crystals of ice on plants. The splitting of wood and the appearance of ice-crystals in the fissures are phenomena well known to tree planters. As a result of the cold, the tissues of the whole plant contract, and consequently their turgescence is much diminished, as well as the permeability of the cell-walls to water. The contents of the peripheral ends of the medullary rays freeze, expand, and are pressed forward, causing a split in the stem at the point of least resistance. The ice forms a layer covering the whole surface of the wound. The internal pressures continue to supply water, by which the ice-sheet is continually augmented.

An ancient oak formed the subject of an interesting exhibit at one of the last meetings of the Linnean Society, when Mr. Carruthers showed some photographs of an old tree which still grows at Conthorpe, in Yorkshire. Prints prepared some hundred years ago show no departure from the normal habit of a fine oak, but recent photos illustrate a remarkable change in mode of growth. The successive ramifications of the thick lower branches no longer spread in all directions, but the latter produce a number of erect shoots giving the appearance of young trees growing from the horizontal main branch, which may be compared with a rootstock sending up vertical shoots, as happens in the Solomon's seal, species of *Polygonum*, and many creeping plants.

German botanists are still busily working at the tropical African flora, to which the recently-issued number of Engler's *Botanisches Jahrbuch* (vol. xix., parts 2 and 3) supplies a further contribution. This includes a large number of new Labiatæ, Orchids, Thymeleaceæ, and others. In the *Journal of Botany* for September, South African botany is represented by a paper by R. Schlechter, who describes some new species of Asclepiadaceæ from Natal and elsewhere.

Some time ago we gave an account of Magnin's work on the flora of the Jura Lakes. During the summer of 1893, under the auspices of the Michigan Fish Commission, the flora and fauna of Lake St. Clair have been studied in a similar manner. Mr. A. J. Pieters, the botanist of the party, states that the flora was found arranged in zones, limited by the depth of the water, each with certain characteristic plants, but Magnin's Nupharetum did not exist.

News has arrived from Mr. G. F. Scott Elliot, in a letter dated Ruwenzori, the 24th of May, in which he states that he has been on the mountain for nearly two months, collecting and exploring under

somewhat unfavourable conditions. In spite of fever, which has kept him to his bed for half the time, Mr. Elliot has been able to get to 11,000 feet, where he found a violet and a tree heath. A deciduous forest extends from 7,600 to nearly 9,000 feet, above which are bamboos. The forest is damp to a degree, a cloud usually hanging over it the whole day, making the processes of collecting and drying rather difficult.

The English Dialect Society is anxious to include the supplement to the *Dictionary of English Plant Names* among its publications for 1894. Those who have any additional names or notes are invited to send them as soon as possible to Mr. James Britten, 18 West Square, Southwark, London, S.E.

A collection of twenty-three human skulls made by Dr. Machon in the caves and old cemeteries of Patagonia, is the subject of a detailed illustrated description by Dr. R. Verneau in the last number of *L'Anthropologie* (vol. v., pp. 420–450). Seven specimens have been presented to the Paris Museum of Natural History. Dr. Verneau confirms the conclusion already arrived at by Dr. F. P. Moreno, that several distinct races of men existed in Patagonia before the arrival of Europeans. It is, however, still impossible to decide definitely whether any or all of these races lived in the country simultaneously, or whether they flourished successively at different periods. Some of the artificially deformed skulls are curious.

The members of the energetic Field Naturalists' Club of Trinidad are continuing to publish valuable annotated lists of the fauna and flora of the island. We have just received the latest of these lists, dealing with the Reptiles and Batrachians, compiled by Messrs. R. R. Mole and F. W. Urich, with the assistance of Dr. O. Boettger and Mr. G. A. Boulenger. De Verteuil's work on Trinidad enumerates twenty-six reptiles, but the species of most of these are not to be recognised, except by the local names. Messrs. Mole and Urich are more scientific, and the list before us is only preliminary, to be followed by a series of notes on the life-history of the various species. There are six tortoises, twenty-five lizards, thirty-three snakes, and twelve batrachians, amounting in all to seventy-six forms, of which twenty-one are recorded for the first time. Dr. Boettger describes one new gecko (*Sphærodactylus molei*) and a frog (*Hylodes urichi*).

Dr. W. H. Dall has been investigating the genus *Gnathodon*, Gray, a series of bivalve mollusca inhabiting subtropical America in the gulfs of California and Mexico, and preferring, like oysters,

brackish to either fresh or salt waters. By Gray and Desmoulins this genus was referred to Mactridæ, while the fact that in certain features it recalled Cyrenidæ was duly recorded. Fischer, in his "Manuel," under the synonym of *Rangia*, places it next to Cyrenidæ, at the same time raising it to family rank. A careful examination of the animal, however, has convinced Dr. Dall that the earlier naturalists were right, and the genus must be classed with the Mactridæ. Six genuine and six spurious or doubtful species are recorded in this monograph, which appears in the *Proceedings* of the U.S. National Museum, xvii., pp. 89–106; 1 pl.

Mr. R. W. Chapman and Captain Inglis are continuing their valuable observations on the tides of Port Adelaide, South Australia, and we have just received their brief report presented to last year's meeting of the Australasian Association. At that time they were proceeding with a second analysis of the Port Adelaide curves, with the aid of Professor G. H. Darwin's computing apparatus.

The lake formed a year ago by the great landslip at Gohna in the Himalaya, burst through its dam at the end of August, and thus has terminated one of the most gigantic geological catastrophes of recent years. Mr. T. H. Holland, of the Indian Geological Survey, has favoured us with a copy of his detailed illustrated report (*Rec. Geol. Surv. India*, vol. xxvii., pp. 55–65, with 5 plates and 2 maps), and from it we learn that the slip was due to the ordinary cause. The rocks on the side of the valley which collapsed dipped at a high angle towards the valley, and the landslip naturally followed heavy rains. On a small scale, as Mr. Holland points out, such valleys as that of Cheddar illustrate the same principle; the side on which the rocks dip towards the valley has a very gentle, stable slope, while the opposite side stands equally firm as a great precipice.

We are informed that a boring for coal will shortly be undertaken in the valley of the Stour. At a meeting held in Ipswich on the 28th August, the Chairman of the Directors (Mr. R. C. Napier) intimated that over £5,000 had been promised, so that there was no reason for further delay. It will be remembered that slaty rocks, older than Coal-measures, but of Carboniferous age, have been pierced at Harwich, and it is considered likely that coal basins may occur in the folds of the old rocks between the east coast and London.

Geologists of the rising school are continually attacking the orthodox theory of the origin of coal-seams; and it is quite likely they are correct in most cases, though the exceptions to the rule of growth *in situ* are never proved to be more than local. Another contribution

is made to the subject by Mr. Herbert Bolton in the last part of the *Trans. Manchester Geol. Soc.* (read June 12, 1894). He describes remains of plants and fishes from the Jarrow Colliery, Co. Kilkenny, Ireland, and concludes with an explanation of the well-known fact that a good deal of the anthracitic coal in this region does not rest upon the ordinary fire-clay, but upon a black shale. He considers that the coal thus situated represents the overflow of peaty matter from a lagoon in the neighbourhood, and thinks the case may be analogous to that of the bursting of a modern peat-moss.

FOR the past thirteen years Dr. Hermann Credner, Professor of Geology in the University of Leipzig, has contributed an important series of memoirs to the *Zeitschrift* of the German Geological Society, on the primitive batrachia and reptiles met with in the Lower Permian Formation near Dresden. The series being now apparently completed, he has just conceived the happy idea of presenting to those who have received the reprints a title-page and table of contents to preface the volume the memoirs will form when bound together. There is a list of eleven species of Stegocephali, mostly new, and the reptiles comprise the two important forms, *Palæohatteria* and *Kadaliosaurus*. Dr. Credner's very fine series of careful drawings are only rivalled by those of Dr. Anton Fritsch, in his well-known *Fauna der Gaskohle*, and the one work supplements the other.

WE have received from Messrs. Dulau & Co. a catalogue of Zoological works, Mammalia, which contains some 2,500 entries.

I.

The Origin of Species without the Aid of Natural Selection.

A REPLY.

I AM much indebted to Mr. Wallace for his interesting paper (NATURAL SCIENCE, vol. v., p. 179). As he was the joint propounder of Natural Selection with Darwin, I could not hope for a weightier critic. Still, I am not in the least shaken in my opinion by it.

In reply, I would first observe that I take the terms "definite" and "indefinite"—which Mr. Wallace says he does not understand—from Darwin himself, who says: "The direct action of changed conditions leads to definite or indefinite results;"[1] while of the former he writes: "By the term definite action, I mean an action of such a nature that, when many individuals of the same variety are exposed during several generations to any change in their physical conditions of life, all, or nearly all the individuals, are modified in the same manner. A new sub-variety would thus be produced without the aid of selection."[2]

These words really strike at the root of Darwin's theory; and, indeed, the whole of my contention, if it were not founded on facts and observations, might be based on this passage; for Darwinism may be compared to an inverted pyramid, the apex being the *mistake* Darwin made in supposing variations in any seedlings of a plant (or variety) in *nature* being "indefinite." *They are always definite.* Though hundreds may perish, *the survivors all vary in the same direction*, viz., towards adaptation to the environment.[3]

In a correspondence with the late Professor Romanes last spring on this subject, he wrote me as follows: "Of course, if you could prove that indiscriminate [*i.e.*, indefinite] variations have not occurred in wild plants, but only under cultivation, you would destroy Darwinism *in toto*." (Hyères, March 12, 1894.)

Having stated my case thus briefly, I will proceed to remark upon Mr. Wallace's criticisms.

1 "Origin of Species," 6th ed., p. 106.

2 "Animals and Plants under Domestication," ii., p. 271.

3 See, *e.g.*, "Origin of Species," pp. 72, 175, 176.

Mr. Wallace writes: "It is, of course, admitted that direct proof of the action of Natural Selection is at present wanting." "At present"—why is it still wanting if it really exist? Has not one of the many biologists who have studied nature all over the world, during the last five-and-thirty years, been able yet to find one single proof?

On the other hand, I venture to say and to prove, *in the strictest sense of the term*, that Natural Selection is not wanted as an "aid" or a "means" in originating species.[4]

In the *elimination* of superfluous weaklings, in the *delimitation* of specific forms, and in the *distribution* of plants, Natural Selection may be largely credited with the results, but in the *origin* of species it is not wanted.

Darwin says that "Natural Selection has no relation whatever to the primary cause of any modification of structure"[5]; and the question with which I am solely concerned is to try and find out how and by what means variations in structure originate in plants; for new sub-varieties, varieties, sub-species, species, and genera are all based upon morphological variations; these being the only things systematic botanists trouble themselves with at all. *Then*, whether Natural Selection exists as a "means" or an "aid" in establishing these differences is a separate question altogether, as Darwin insists. To answer *this*, one looks to see, not only if Nature supplies those data upon which Natural Selection is supposed to act, but if they are of any use in the process. Mr. Wallace tells us what they are, for he says: "Offspring resemble their parents very much, but not wholly—each being possesses its individuality. This 'variation' itself varies in amount, but it is always present, not only in the whole being, but in every part of every being. Every organ, every character . . . . is individual; that is to say, *varies* from the same organ, character . . . in every other individual."[6] Now, is there any evidence, direct or indirect, that any such slight morphological differences as are here alluded to are of the slightest consequence to a *seedling* so as to enable it to survive in the struggle for life? What attempts have been made experimentally to test the truth or the reverse of this hypothesis?

Let it not be forgotten, too, that specific and generic characters are more often taken from the flowers and fruits, organs which are totally undeveloped when the "slaughter of the innocents" takes place, and, therefore, must be all put out of court so far as Natural Selection is concerned in bringing about the survival of the fittest. It has been suggested that a plant survives because, say, of some superiority in the structure of the flower, this feature being correlated with a more vigorous constitution than that of the other seedlings, which die in a premature state. I reply this simply begs the question,

[4] The title of Darwin's book is "The Origin of Species by means of Natural Selection."

[5] "Animals and Plants under Domestication," ii., p. 272.

[6] "On Natural Selection," p. 266.

or is putting the cart before the horse. *A seedling survives solely because it is vigorous.* This is capable of proof, and whatever flowers it may subsequently bear, it must be contented with them, whether they be the "best" or not for fertilisation or otherwise. In corroboration of the above, I would add my own experience with small and large seeds. These show that the better nourished have a much greater chance of starting and crowding out the rest by growing into larger plants, and that if small seeds be selected for some years, they either die out altogether or a tiny race of beings is for a time procured. Hence, for the word "fittest," *i.e.*, morphologically, I would substitute "strongest," *i.e.*, constitutionally.

I note here that Mr. Willis says (NATURAL SCIENCE, v., p. 240) that "Natural Selection has to be *dis*proved." No one, however, can be called upon to "prove a negative." It is for Darwinists to prove that the Origin of Species *does* really require the aid of Natural Selection.

On the other hand, it is for me to prove that the Origin of Species can take care of itself; in other words, to establish the truth of Mr. H. Spencer's observation: "Under new conditions the organism immediately begins to undergo certain changes in structure, fitting it for its new conditions,"[7] and that what is true for the individual is true for its offspring, the result being, to adopt Darwin's words, a new sub-variety without the aid of Natural Selection is produced.

I will now give illustrations of "definite" and "indefinite" variations. In 1847, Professor J. Buckman sowed seed of the wild parsnip in the garden of the Agricultural College at Cirencester. The seedlings began to vary, but in *the same way*, though in different degrees. By selecting seed from the best rooted plants, the acquired "somatic" characters of an enlarged root, glabrous leaves, etc., became fixed and hereditary; and "The Student," as he called it, having been "improved" by Messrs. Sutton & Sons, is still regarded as "the best in the trade." This is *definite variation*, according to Darwin's definition, for those weeded out did not differ from the selected, morphologically, except in degree, the variations towards improvement not being quite fast enough to entitle them to survive.

M. Carrière raised the radish of cultivation, *Raphanus sativus*, L., from the wild species *R. Raphanistum*, L., and moreover found that the turnip-rooted form resulted from growing it in a heavy soil, and the long-rooted one in a light soil.[8] Pliny records the same fact as practised in Greece in his day, saying that the "male" (turnip form) could be produced from the "female" (long form), by growing it in "a cloggy soil." Both forms are now, of course, hereditary by seed.

When a plant has been *long cultivated*, the relatively fixed nature, characteristic of most wild forms, generally breaks down; and the seeds from one and the same individual plant cannot always be

[7] "Factors of Evolution."

[8] This has been corroborated by M. Languet with the carrot. *Soc. Roy. et. Cent. d'Agricult*, 2nd ser., vol. ii., 1846–7, p. 539.

depended upon "to come true." Thus, an eminent agriculturist once said to me (a trifle hyperbolically, of course) speaking of the varieties of wheat: "You can almost get a different variety from every grain in a single ear."

Sir J. D. Hooker records no wild variety of the cabbage (*Brassica oleracea*, L.). Theophrastus (300 B.C.) only knew three cultivated forms. Pliny speaks of six, but who will count them now? It would seem as if plants underwent two courses of variation. First, in adaptation to it, by responding at once to a new environment, *i.e.*, definite variation. Then, when this has been thoroughly established, as with all of our ordinary vegetables, they may vary indefinitely, but why they do so no one can tell. Still, taking a broad view of the whole process, it is obvious that all such variations were primarily due to the environment of cultivation; because *they never occur in the wild state.*

Hence, to test the reality of specific characters of wild plants, as Mr. Wallace describes, by their degree of stability under cultivation in a garden, cannot possibly give but the most untrustworthy results. Some may resist for a time the influences of the new artificial environment, others may succumb to them; but it will be *the very best means of forcing them to change*; for, as Darwin and Weismann assert, cultivation induces variability. Suppose this test had been supplied to the wild and tall *Cineraria cruenta* with its small flowers; what would a systematist now say if he had never known the origin of the modern dwarf kind with large flowers of innumerable colours? He would undoubtedly call it a new species.

The rule may be laid down that *a species may be constant as long as its environment is constant, but no longer.* I have changed the spiny *Ononis spinosa*, L., the Rest-harrow, both by cuttings and by seed, into a spineless form undistinguishable from the species *O. repens*, L., in two years; but it would have, I doubt not, at once reverted to *O. spinosa* if I had replanted it in the poor soil from which I took it. It seems, therefore, to be a very hazardous and fallacious method of testing the value of specific or other characters by cultivation. A wild plant may or not change at once. Thus the carrot, *Daucus Carota*, L., proved refractory with Buckman, but not with Vilmorin, who converted this *annual* to a hereditary *biennial*, by sowing the seed late in the season, till the character of flowering in the second season became fixed.

Indeed, the proposed test is not unlike trying a man's guilt by making him eat an ordeal bean!

Mr. Wallace illustrates his remarks by the case of species of *Arabis*, but quite fails to perceive that it goes to prove my contention altogether. He says: "*A. anachoretica* has tissue-papery leaves—*due to its growth in hollows of the rock*" (my itals.). "Seeds of this plant, when cultivated at Kew, produced the common species *A. alpina*. The same thing occurs with many plants, as every cultivator knows."

If the rocky environment is to be credited with species-making in the one case, so must Kew be in the other. In both cases there is neither mention made nor need of any selection at all. Mr. Elwes told me that the various bulbous plants he introduced from the East into his garden at Preston, Cirencester, changed so greatly in a few years in all their parts that he could scarcely recognise them again.

Mr. Wallace adds: "Other forms, with no greater peculiarities externally, preserve their characters under cultivation, though exposed to the most varied conditions."

This is equally and quite true; but any investigation into the causes of the origin of species by *variation* has nothing to do with any other question of the causes of preservation of the type-characters, or *heredity*. Evolution accounts for all living beings by variation; but it does not attempt to offer any explanation of the existence of "survivals." *E.g.*, *Nautilus* and *Lingula* have lived on from the Silurian days till now; *Equisetum* has flourished from, at least, the Carboniferous epoch till to-day. Therefore *change is not absolutely necessary in organisms under changed conditions;* but when it does occur, then I maintain, with Dr. Weismann, that all changes are primarily due to external influences. He says: "We are driven to the conclusion that the ultimate origin of hereditary individual differences lies in the direct action of external influences upon the organism."[9]

Mr. Wallace is good enough to call attention to my book, "The Origin of Floral Structures by Insect and Other Agencies,"[10] and attacks, very rightly, what I fully admit may be regarded as a weak point in it; *i.e.*, I can bring but few positive illustrations to demonstrate my view that irregular flowers have been formed through the direct action of insects from regular ones; but he quite ignores the whole line of argument running through the book in support of the probability. It is one which Dr. Weismann recommends in support of evolution, which "may be maintained with the same degree of certainty as that with which astronomy asserts that the earth moves round the sun; for a conclusion may be arrived at as safely by other methods as by mathematical calculation."[11] It is the well known argument of the accumulation of coincidences which can furnish probabilities of so high an order that they may be regarded as an equivalent to a demonstration. Thus, physicists tell us that they *know* the composition of the sun, but their knowledge is solely based on the coincidences between the lines of the solar spectrum and those of vapourised substances.[12] Similarly with flowers: when we find innumerable coincidences all tending in one direction, coupled with an indefinite capacity for varying in response to forces in all parts of

[9] "Essays on Heredity," etc. Eng. trans., p. 279.

[10] *International Scientific Series*, vol. lxiv.

[11] "Essays on Heredity," etc., p. 255.

[12] The "fact" that udders have become enlarged by hand-milking is based on a similar accumulation of probabilities.

plants, I still maintain that "Mr. Henslow's theory [does not] utterly break down." Mr. Wallace contends that the negative evidence derived from "regular" flowers, as gentians, tells against me, as they ought to have long ago become irregular, since their "lower petals have been always subject to irritation and have never developed irregular flowers." This is scarcely fair; for not only do all botanists believe—on precisely the same grounds of probabilities—that all irregular flowers have descended (somehow) from regular ones; but that, if he will refer to the chapter on "Peloria," he will see that existing regular flowers, being mostly "terminal," have no "lower" petals at all, but are so situated as to offer access to insects from all points of the compass. Moreover, whenever a plant with normally irregular flowers (which are always situated close to the axis, so that insects can only enter them in one way) produces a blossom in a terminal position (as foxglove, larkspur, horse-chestnut, etc., often do), it at once becomes quite regular. These differences between regular and irregular flowers represent two of those groups of coincidences respectively, to which I referred.

Mr. Wallace adds: "The very first essential to this theory is to prove that modifications produced by such irritations are hereditary." Quite so. But this proves itself, if my contention be right; for plants with irregular flowers *are* all hereditary. So that there is no need to prove this point, provided the "previous question" as to the origin of irregular flowers themselves be answered. But the converse change can be readily shown; for flowers, normally irregular in nature, often revert to their ancestral regular form under cultivation in the absence of insects, and then come true from seed, as do Gloxinias. Unfortunately, one cannot make a regular flower become irregular. How long it required in nature to do so no one can tell; but all the innumerable minute details of structure coincide to one end; a multitude of correlations all fit together for one effect; so that we may put the alternative thus—Which is more likely, that some one common cause has set up these minute, often microscopic, details in unison together; or that they have arisen by selection out of innumerable wasted variations, which no one ever saw in nature, nor can even ever see a trace of under cultivation?

When, however, we come to variations in the vegetative system of plants, there is nothing easier than to prove, first, the direct action of the environment, and secondly, the hereditary persistence of the result. I need go no further than to take Buckman's parsnip, Carrière's radish, Vilmorin's carrot, or anybody's variety of cabbage. What are all these and many other instances but experimental verifications.

Mr. Wallace alludes to my last paper on "The Origin of Plant-Structures by Self-Adaptation to the Environment, exemplified by Desert and Xerophilous Plants,"[13] and attacks my inferences with

[13] *Journ. Linn. Soc. Bot.*, xxx., p. 218.

regard to spinescent processes of desert plants; but he again ignores the primary argument of innumerable coincidences; while in the case of vegetative organs this argument has been in many cases "verified by experiment." When, however, Mr. Wallace calls in question my statement that spines are correlated with a dry soil and atmosphere, he controverts those of Belt, Aitchison, Scott Elliott, Grisebach and others, for he says: "There is no such general coincidence of aridity of soil and atmosphere with abundance of spiny plants, as very little enquiry will show." Having seen and gathered them myself in the Libyan desert and even on our own sandy heaths, I cannot accept this statement; and if those eminent travellers I have named are misleading us, where are we? He then mentions the Galapagos and other islands, where, though of a desert character, plants are *not* spinescent. Here, again, I am not concerned with what does *not* occur, but with what *does*. Moreover, any cause that may tend to arrest an axis likewise may tend to render it spinescent, and more than one cause may produce the same result,[14] so that it is not altogether strange to find spinescent processes away from deserts; but I *do* maintain that spinescence is one and an important element in the *facies* of hot and arid deserts with a barren soil.

Mr. Wallace advances the well-worn theory of the interaction of mammals and spines. In the first place, if I may still believe in the prevalence of spines in deserts, they occur where no herbivorous quadrupeds live. Secondly, if a mammal wishes to eat a spiny plant, it somehow often gets over the difficulty; thus donkeys knock off the spines of *Opuntia*; horses eat gorse. I had a cow which was partial to holly, another rejoiced in nettles! But all this is beside the question. It seems to me that there is a lurking element of teleology in this view: for any structure which arises *in anticipation of its use* savours of natural theology[15] rather than of evolution by natural processes alone. I fully admit that plants, when once they have got their spines, may be able to keep animals more or less at bay; but they originate, I maintain, as a mere accidental and inevitable result of an arrest of the organ in question, such arrest being mainly due to drought.

If teleology in its old dress of *Design in anticipation of Use* is, and ought to be, extinct, we may accept Darwin's form of it, that Evolution is the Deity's method of creation. Let us, then, recognise protoplasm as having been impressed with the power of self-adaptation—such being the inference from direct observation of its behaviour; and, consequently, enabled to build up structures in an automatic response to the environmental forces, whenever it is necessary to bring about a better degree of equilibrium between the internal and external forces.

[14] I observe Mr. Osborne makes a corresponding statement. NAT. SCI., p. 223.

[15] Indeed, such anticipation is absolutely necessary for the theory of Natural Selection in general.

On the last page but one of his paper, Mr. Wallace alludes to the case of the hard shells of nuts, and asks if the direct agency of birds, monkeys, etc., has anything to do with them. He admits the question is absurd. I do not therefore know why he asks it. I have not myself written a line on this branch of the subject, but will suggest, from what one knows of all other parts of plants having the capacity of varying, that I see no reason for inferring that hard coats of fruits should be subject to any different law. Soft fruits vary readily enough, as melons, pea-pods, apples, as well as pears in their degrees of "stoniness." Moreover, under cultivation, varieties of forms of nuts and walnuts have arisen, as well as of olives, almonds, and dates, and other hard-coated or hard-seeded fruits. The fact seems to be that cultivation affects the whole organisation of the plant; for the environment is not always solely concerned with an isolated bit of a plant, as a nut or a root. Many visible changes are due to secondary causes within the individual; but in all cases, as I believe with Dr. Weismann, they are *primarily* attributable to the direct action of the environment, simply because *they never occur unless the environment itself is changed.*

Finally, to return to my starting point. The whole question lies within a very small compass. Thus, first, no one disputes the fact that the environmental forces can act upon an organism. Secondly, that the organism can respond to those forces. But now follow two views. Darwinites say that the resulting variations are indefinite in Nature, just as they so often are in cultivation; and that the environment *selects* the best fitted *to survive.* I say that they are always definite in Nature: and not only exceptionally so, as Darwin thought; and that the environment *induces* the best fitted *to arise.*[16] Therefore, Natural Selection has nothing to do in aiding the Origin of Species.

For additional facts I would refer the reader to a paper entitled, "A Theoretical Origin of Endogens from Exogens, through Self-Adaptation to an Aquatic Habit"[17]; and to a companion volume to the "Origin of Floral Structures," which I hope will be shortly published in the "International Scientific Series," and entitled "The Origin of Plant Structures by Self-Adaptation, in Response to the direct Action of the Environment." In this, similar lines of argument, with illustrations, will be applied to Desert, Aquatic, Maritime, Alpine, and Arctic, as well as Climbing Plants, and to the Origin of Peculiarities of Roots, Stems, and Leaves.

George Henslow.

[16] See "Animals and Plants under Domestication," ii., p. 272.

[17] *Journ. Linn. Soc. Bot.*, xxix., p. 485.

II.

Horticultural "Sports."

THE immense variety existing at the present day in the cultivated flowers, foliage plants, fruits, etc., which figure in our horticultural shows, while evoking the admiration of the many, is rarely appreciated at its true value as the outcome not only of infinite labour in culture and careful selection, but as largely due to vagaries and freaks of nature quite outside the scope of any known laws. Thus, we see flowers such as tulips, hyacinths, dahlias, and others exhibiting all, or nearly all, the colours of the rainbow in conjunction with most complex variations in the form of the blossoms, all of which have been developed from single-flowered normal wild plants, with one-coloured petals. Now, a large part of this development is undoubtedly due to the careful selection and accumulation, generation after generation, of comparatively small differences which appear in the seedlings, but it is a remarkable fact, of such general occurrence as almost to constitute a law, that when a wild plant is brought under culture and thus subjected to more or less artificial surroundings, it becomes in some way subtly modified, so that not only is its offspring likely to vary more widely than before, but even individual buds of the plant itself may produce modified foliage, flowers, or fruits capable of being propagated and so forming a new and permanent variety. Probably the most striking instances of this are seen in the origin of Nectarines and their varieties. These have all sprung direct as perfect nectarines from peach trees or peach stones. Old peach trees, after producing peaches only for many years, have produced fruiting branches bearing true nectarines, and since nectarine stones in their turn may yield true peaches, it would seem that it is really a case of dimorphism, akin to the two forms of leaves sometimes seen in plants. This phenomenon has repeatedly occurred in the peach, most of the many varieties of which have been raised from stones, though some have originated by buds, and it is curious that in a few of these cases the fruit has only varied in the direction of being earlier or later in ripening—a feature which is constant. A well-authenticated case is recorded of a gooseberry bush being so sportive in its buds that four widely different sorts were produced upon it. Cases of this description, however, bring us to the question of cross-fertili-

sation, by which means the horticulturist often succeeds in blending or modifying the colours and forms of two very different varieties or even species, one result of which is a tendency to revert, and the gooseberry in question may have owed its diversity to this. The tuberous Begonia, for instance, which now forms one of the most salient features of our shows, began its upward career with a blend of two distinct species. This blend, in its first results, yielded plants of very different habits to either parent. Fortunately, the alliance was a fertile one, hence propagation by seed and selection of offspring presented no difficulty. It was speedily seen that the two species were sufficiently at variance in the blood of their joint progeny to induce an immense divergency of type, and seed being sown from the most diverse forms, the final result was that no two of a batch were quite alike. Here, of course, was the cultivator's opportunity, and in the hands of Mr. J. Laing and Mr. H. Cannell, in the course of twenty years most magnificent strains of huge double and single flowers, of all habits and all tints of white, salmon, and red, have culminated in constituting the naturally insignificant begonia a fair rival to the very Queen of Flowers herself, barring her perfume. Here is, indeed, a triumph of Art over Nature, so far as the art of selection is concerned.

What, however, the public do not see, though they feel it in the high prices necessarily demanded for the results, are the thousands upon thousands of worthless plants which accompany the good ones. Often and often, and especially in the earlier stages of development, the cultivator has to raise to blooming point, or, worse still, in the case of trees, to fruiting point, an enormous number of seedlings in which he may vainly search for a "break" in the right direction. This "break," obtained, however, he starts under ever-improving auspices, until—as in the case of Begonias in question—he finds his best flowers barren, their extreme development being at the cost of their reproductive energy.

Then, too, it is a rare thing indeed for the breeder to be perfectly satisfied. Frequently, when his flower is perfected as regards the blossom beyond his utmost expectation, the stalk is too short to display it properly or too weak to keep it upright. A heavy double flower with a pendulous habit will not do. It must look you boldly in the face and display its charms effectively. In time this is generally arrived at by judicious crossing, or it may be by simple selection of seedlings, provided always the utterly barren stage has not been reached.

Fashion plays a large *rôle* in horticulture. Dahlias, for instance, starting from a simple single starlike bloom were transmuted by long years of selection into huge spherical masses of regularly folded petals, and decorated with any colour desired except the unattainable blue. Suddenly the æsthetic craze steps in with the sunflower as its floral deity, and lo! the dahlia must be single and the cultivators

have an opportunity of marketing their wastrels, at the expense, of course, of their choicest. Then the Juarez or Cactus type turns up from Mexico, and becomes the rage, and in a season or two we find it evolved in all tints, and largely from the old forms, many being christened Cactus, which, in the breeder's opinion, had simply "gone wrong," and, but for the fashion, would have decorated the dust-heap.

So far, we have touched mainly on the minor "sports," which need to be accumulated to be striking; but in many cases, as in that of the nectarines, among the fruits we owe some of our best flowers to wide and sudden departures from previous types. The beautiful double crimson hawthorn, for instance, appeared as a bud-sport on a pink one, a branch resulting which was entirely covered with dark crimson flowers. Many of our best roses we owe to bud-sports. The white moss-rose came as a sucker on the red, and, when severed for propagation, the striped moss-rose sprang from a bud near the cut. One variety of chrysanthemum yielded, in the same way, no less than six distinct forms. Generally, varieties originating in this manner remain constant when propagated by cuttings, but are very liable to revert when reproduced by seed. Variegated forms innumerable have originated as bud-sports on normal green plants, and these, when cut off and propagated by buds, retain their peculiarities. The writer recently saw a plumose form of *Polystichum angulare* of which six fronds out of seven were profusely splashed with pure white, yet the plant was several years old, and neither it nor its parent, of which it was an offset, ever showed the slightest tendency to variegation before. This case is very peculiar, so much of the plant being suddenly affected, and to such an extreme extent. Seeds of variegated sports of this class generally produce normal green and wholly white plants, the latter of which are very short-lived, owing to the absence of chlorophyll. In very rare cases, a graft has been known to affect the stock, and form a graft-hybrid. *Cytisus Adami*, a hybrid laburnum, is, perhaps, a unique oddity in this way, being a quasi combination of a purple and a yellow laburnum. The leaves of each species differ materially, as does the general habit of growth, and in specimens of this, which may be seen at Kew, the tree is constantly sending out branches and twigs of each distinct species side by side, and mixed up in the most curious fashion; the foliage and habits peculiar to each accompanying the characteristic bunches of yellow or purple flowers.

The well-known Cockscomb, a densely fasciated form of *Celosia*, is ordinarily a rich crimson, but at a recent show of the Royal Horticultural Society, one grower who had made a hobby of this for years exhibited fifteen distinct colours of it, including a yellow, and every plant was furthermore crimped and crested in a fashion of its own. Patience is indeed a virtue to the selective cultivator. Tulips, to wit, frequently remain for many years in their "unbroken" stage. The seedlings of this family, when they first bloom, present very different

characters from the parental form, but after a time they "break" or assume their proper character quite suddenly, and until this occurs it is impossible to say whether they are valuable or not. Orchids owe their vast variety chiefly to unaided Nature, and the efforts of skilled travellers who ransack their native habitats to find new ones. Of late, however, much has been done by crossing allied species, and so obtaining flowers embodying their united charms. Imported Orchids form somewhat of a lottery, and many a prize has been acquired unwittingly in the shape of a dormant pseudo bulb, costing a few shillings, and fetching, when its true character has appeared in its bloom, a handsome sum in guineas. Such a case occurred not long since, when an orchid was put up in a thumb-pot, with a tentative reserve of a few pounds, and was knocked down, after a spirited competition, at one hundred and forty guineas. Such plants may, of course, be new species, but usually they are varietal forms of well-known ones, and are thus simply natural "sports" of precisely the same character as those already described.

Chas. T. Druery.

III.

On the Geology of the Plateau Implements of Kent.[1]

THIS subject has been fully treated by Professor Prestwich in his several memoirs in the *Quart. Journ. Geol. Soc.*, vol. xlv., 1889 (the old worked flints at pages 282–286); vol. xlvi., 1890 (the general geology of certain drift-deposits, and the geological stages of their formation, pages 166–168 and 179); vol. xlvii., 1891 (the same subjects, pages 126–159, particularly pages 157–159, and plate 6, and the peculiar flints at page 160, with plate 8). Also in the *Journ. Anthrop. Instit.*, vol. xxi., 1892, pages 246–270, with plates 18–21.

Mr. W. J. Lewis Abbott has more lately contributed to NATURAL SCIENCE (vol iv., no. 20, April, 1894) an interesting paper "On the Plateau Man in Kent," containing a clear account of the old rude implements found on the Chalk Plateau, illustrated with two plates.

It is shown in these memoirs that certain superficial soils on the North Downs, between Sevenoaks and Rochester, contain numerous rudely-worked flints, which are connected with a gravel area, of very great antiquity, and probably derived originally from the side of the old Wealden Range, which once rose 2,000 to 3,000 feet above, where Crowborough and other hills in Sussex now are. Those heights were long since removed by natural agencies, such as rain, rivers, sea, frost, and ice, probably with more than one time of energetic action, and portions of their *débris* were distributed by old streams or otherwise to and over a plateau of Chalk at a lower level on the flanks of the range.

These rude old implements have an ochreous tint, and are associated with limited patches of much-worn gravel having the same colour, and not forming well-defined beds, by no means thick enough for the making of a gravel-pit. Nor are the implements under such conditions as would lead to the supposition of their having received

[1] This paper was read before a combined meeting of the Anthropological and Geological Sections of the British Association, on August 10, 1894, in consequence of a suggestion made by the Anthropological Institute that it was desirable to know more of "The evidence afforded by the 'Plateau Gravels' relative to the Antiquity of Man."

their staining in their present position. A few only have been found beneath the surface.

It is very desirable that excavations should be made at proper places on the plateau to discover, if possible, the extent and thickness of the implementiferous soil.

The peculiar reddish-brown colour of these rude implements, with ochreous stain and some adherent particles of limonite, was probably obtained when they were lying in an old ferruginous gravel in their original place; and some of this gravel has accompanied them to the plateau. Their colour and greater wear distinguish them from the few palæolithic and neolithic worked flints found on the plateau. Some have a strong resemblance to the "valley forms," and were most probably transitional in character and condition. Many are very much water-worn—an indication of their long and distant travel. Some are *scratched*,[2] by very close pressure, either with or without the intervention of ice-action.

The evidences borne by these peculiarly-shaped flints of their having been fashioned by the hand of Man, have been carefully dealt with by Professor Dr. Prestwich (see above), Messrs. B. Harrison (their discoverer), De B. Crashaw, W. J. Lewis Abbott, A. Montgomerie Bell, and others. Mr. J. Allen Brown has succinctly referred to them as "Eolithic: roughly-hewn pebbles and nodules, and naturally broken stones, showing work, with thick ochreous patina; found on the plateaux of the Chalk and other districts, in beds unconnected with the present valley-drainage"; and "under conditions which clearly indicate that they are older than the usual valley-drift implements." (*Journ. Anthrop. Instit.*, vol. xxii., 1892, pp. 93, 94).

In "The Ground Beneath Us," 1847, pages 71–79, many interesting points bearing on this subject were already illustrated and explained by Professor Prestwich. The great changes of land-surface in this part of the European area were referred—firstly to the movements, accompanying the elevation of the Pyrenees, when the Chalk became dry land, with its uplands, valleys, and estuaries; and he showed that at that time the Wealden area formed an island in the Thanet-Sands Sea.

Some rivers afterwards brought down the clays and sands which now constitute the "Woolwich and Reading" beds. With some submergence other Tertiary beds were formed, probably extending over a part of what is now the Wealden area.

Secondly, this area, with its stratified coatings, was raised up (after the formation of the London Clay) by movements accompanying the elevation of the Alps by the lateral pressure caused by the earth's contraction. The other Tertiary beds (of Paris, Bracklesham, etc.) had helped to shallow the open sea surrounding the island of the

[2] On one and the same specimen are some scratches quite raw, and some that have been coloured with red ochre, also some merely linear streaks of red hæmatite.

Weald, which was ultimately to be an elevated, elliptical, weather-worn, and sea-eaten dome of great height.[3]

The successive stages were :—

1. When the elevation of the Wealden area attained its maximum, there was certainly a considerable thickness of Chalk on the surface, and this was necessarily exposed to marine and atmospheric denudation.

2. The immediate result of this was the wearing-away of the

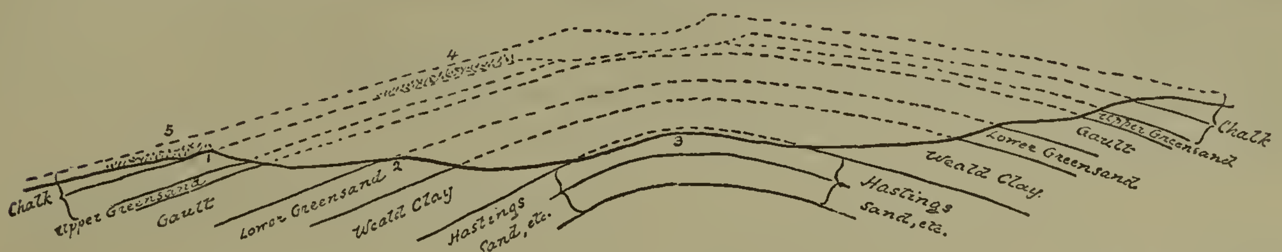


FIG. 1.—Diagram showing roughly the relative position of the formations constituting the Wealden Anticlinal between the North and South Downs (1. Limpsfield, 876 ft.; 2. Oldbury, 620 ft.; 3. Crowborough, 803 ft.); also their successive denudations; and the original place of the Old Gravel (4), some of which was brought down by natural agencies to the Chalk Plateau (5) now existing.

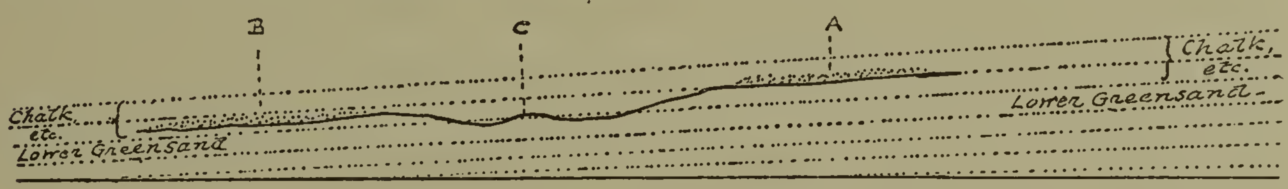


FIG. 2.—Diagram showing the possible position of the Lower Greensand outcrop (*C*) when the Old Gravel (*A*) was being transferred from the higher (*A*) to the lower (*B*) level of the Chalk by natural agencies along a continuous surface. The strata are set at too high an angle in Fig. 1 to show what is required here.

(*A*) Old Gravel in place. (*B*) Plateau Gravel derived from the Old Gravel. (*C*) Outcrop of the Lower Greensand.

Chalk, and the trituration of the washed-out flints; and thus the formation of a great bed of shingle—that of the Thanet Sands.

3. With continued wave-action, these shingle-banks were washed away and distributed at a lower level, to be the Pebble-beds of the "Woolwich and Reading" series, which had been formed in the meantime by rivers from the hill-ranges. The pebble-beds can now be seen at Addington, Blackheath, etc., extending to the very edge of the present Chalk escarpment.

4. The deposits of this stage were next removed in part; and in course of time the Diestian beds were laid against the flanks of the lowered and perhaps sinking range. Of these strata some limited patches, such as the Lenham beds, remain here and there on what are now the Chalk Downs.

5. One or more accumulations of Chalk-flint *débris* were formed

[3] A neat little diagrammatic section across the Wealden area, showing the high elevation of the anticline (about 2,000–3,000 feet; Topley, *Mem. Geol. Surv. Weald.*, 1875, p. 217), was published in Professor (Sir A.) Ramsay's "Lectures on the Physical Geology and Geography of Great Britain" (five editions, 1863–1878). At the time of this elevation of South-eastern England, the Ardennes attained a height of about 18,000 feet, as shown by MM. Cornet and Briart, *Mém. Soc. géol. Belgique*, vol. iv., p. 71.

at about this stage of the history of the old Wealden Range or Island, and were probably characterised by the presence of iron-oxide in greater quantity than in the common ferruginous gravels of the South-east of England at present. Man, being present, used such pieces of the flint as suited his requirements. Probably, at first, with little or no alteration of their form; but afterwards he applied them with definite modification of their shapes to meet his wants in killing, skinning, cutting, fire-making, rubbing, pounding, scraping, drilling, knocking, breaking, chopping, digging, etc., that is, in tooling and other processes. Such implements he made and left there, on that old, very old, probably pre-Glacial ground. (*See* Fig. 1.)

6. This gravel extended down the side of the "dome," perhaps tailing down the slopes of uplands by slips, slides, and slushings, probably by more than one stage, after its formation; or, during a succeeding age, the encroaching sea cutting away the lessening dome, or torrents scoring the hill-sides, removed the more or less extensive deposits of ferruginous gravel, with the rude implements left upon it, and spread out the much-worn relics on the slopes of the Chalk below. Here they are now found on the isolated plateau; and they lie on the red Clay-with-flints, that had been in process of formation previously, for ages, by the gradual solution of the Chalk below, and the settlement of argillaceous and sandy matter from the overlying and gradually disappearing Tertiaries. This was co-extensive with the Chalk-surface; and on it lies some of the transported ochreous gravel, together with Tertiary pebbles, less-worn flint-stones, and some *débris* of the Lower Greensand, which the wave-line had then reached. The presence of chert fragments from the Lower Greensand proves that the current of driftage (or the tailing of the gravel) must have passed over the outcrop of the Lower Greensand, and therefore here from south to north, on a continuous surface. (*See* Fig. 2.)

7. Subsequently the outlying Chalk (now the plateau above referred to, sloping from an elevation of about 800 feet on the south, to 400 feet, and less, on the north) was cut off, by denudation in the Glacial Period, from the remaining uplands of the once lofty range; the Holmesdale (or Weald-clay Valley) lying below the escarpment of the Lower Greensand at the foot of the diminished dome, and the Gault Valley at the foot of the Chalk escarpment. (*See* Fig. 1.)

The Diestian or Lenham beds were formed in the early Pliocene period, and the denudation probably began directly afterwards, at about the time of the Red or the Chillesford Crag, in late Pliocene or post-Pliocene times; and as the old ferruginous gravel had not only been formed but had been brought to a lower level before that time, it must be regarded as of pre-Glacial age.

A similar series of occurrences and geological results evidently took place on the south side of the old Wealden uplands, giving origin to the brown-coated rude implements at Friston, near Eastbourne, in Sussex.

The probably great changes of climate, due to rearrangements of land and water and of high and low ground, form part of Professor Prestwich's interesting history of the Wealden hill-ranges, in the sculpturing of which not only aqueous, but glacial agency must have had a large share.

It must have been a great pleasure to the veteran geologist, Professor Dr. Prestwich, to find that his conclusions (in 1890) as to the Pliocene Tertiaries and gravels on the flanks of the diminishing island of the Weald fitted so truly, as consecutive history, with his early views (1847) of the probable conditions of the Wealden dome in Eocene times.

Perhaps it may be asked :—

I.—Why may not the Rude Implements belong to the Red Clay-with-flints, since some few have been obtained in shallow diggings on that bed?

The reply is :—1. Because that clay was not ever moved about, much less *drifted.*

2. Because that clay occurs over other and larger areas without containing any such implements, but only unrolled flints separated from the Chalk by its dissolution.

3. Because the implements are accompanied with chert and ragstone from the Lower Greensand, which *débris* must have come from a higher level—namely, from the outcrop of the Lower Greensand on the side of the hill-ranges of the "Dome," *before* the denudation of the present Gault valley.

4. If the Implements had *settled* down from above *with* the "Clay-with-flint," they must have *either* been in the Tertiaries (!) to be let down, as the Chalk dissolved below (possibly when the Chalk had a somewhat higher slope against the "Dome"),—*or* they must have *been on the* Chalk *before* the Tertiaries were formed there!!

5. The "Red Clay-with-flints" is of local origin, whereas the rude implements, with the sprinkling of brown gravel, are foreign to the place of their occurrence. And on the surface, where these implements are found, chert and ragstone from the Lower Greensand always occur.

6. Professor Prestwich informs me that he has examined hundreds of sections of the "Red Clay-with-flints," and has never found the implements in it. Some of the reputed cases of their presence in this clay are doubtful. In other cases, mentioned by Mr. Harrison (see the list on next page), the depth is so small that the implement may have worked itself down, or may have fallen into a hole.

7. The flints in the red clay are not stained. The crust of the flint retains its white colour; or, if the flint has been broken, the surface of fracture has weathered white. The red clay does not stain; it seems to have removed the water-of-crystallisation, and left a white surface on the flint.

8. Thus both the travel-worn gravel and implements must necessarily have had their source in the flints of Chalk situated at some *higher* level.

II.—Why not belonging to a *local* ochreous drift, deposited originally on the plateau?

The reply is:—1. Because the plateau is out of reach of any present (or late) water-level.

2. Because there is too little of the ochreous gravel there to constitute such a local drift.

Notes.

1.—The old brown "plateau implements" are not found beyond the plateau, except in a few instances when they have been derived from the wear of the plateau, and probably this may be said also of other plateaux.

2.—As to these being *natural* forms, where can any such forms be shown to occur in a river-gravel?

3.—The extremely worn condition of the ochreous flints and implements indicates long-continued movement and great water-wear, if not violent transport; and no well-marked, definite deposit of such a gravel occurs. The tail of the old deposit on the Dome, however, may have extended from the original slopes down to the Chalk level (now the plateau), by more or less violent slides and *débacles*, before the bounding or dividing valleys were excavated in the Glacial Period.

The following is a list (from letters) of the Rude Implements found *in situ* by Mr. Harrison. For the localities, see the Map, *Quart. Journ. Geol. Soc.*, vol. xlv., pl. 9, and *Journ. Anthropol. Instit.*, vol. xxi., pl. 18.

1.—One flake, rude.—From an old sand-pit (now a pond) at Ash.

2.—Three rude implements.—From a trench nearly three feet deep, in sand, with green-coated flints (probably a remnant of Thanet or Woolwich-and-Reading Sands), at Ash.

3.—Two rude.—From a trench in similar sand, 2½ feet deep (for laying in mangold wurzel), at Ash.

4.—Two rude.—From deep-red clay at the Vigo, two finds with an interval of two years.

5.—One rude.—From a hole (for finger-post) in red clay at the Vigo Inn.

6.—One rude—From red clay thrown out in digging a grave at Cudham churchyard.

7.—Several rude.—From trench (for water-pipes), crossing a patch of ochreous gravel on red clay (?), at Dunstall Hill, Shoreham, Kent; three tips of neoliths (?) and many rude implements, all water-worn.

8.—One rude (?).—From a post-hole, 2½ feet deep, at Kingsdown.

9.—One rude.—Fine large implement, at 2½ feet, Ash.

10.—Several rude.—From a deepened dew-pond, near the 775 feet summit level; about thirty rudely-worked flints, and some Oldbury Stone (Lower Greensand).

11.—Some rude.—From a large pipe in the Chalk, filled with deep-red clay, at 720 feet, in an old Chalk-pit, a little below the crest of the escarpment, some deep-tan-coloured implements.

12.—Some rude.—From the grave for a horse on the crest of the escarpment, at about 775 feet; the surface soil was opened out for five feet, and "old implements" were found in the clay and gravel thrown out.

Hence it can be seen that there is evidence of the existence of the Rude Implements locally beneath the surface, and that systematic excavation is required.

T. RUPERT JONES.

IV.

The Effect of Temperature on the Distribution of Marine Animals.

THE question of the influence of temperature on marine animals is a comparatively old one. Twenty years ago and more it had been already discussed in England and America, but even at the present day it is far from being solved. On the contrary, very conflicting views prevail as to the importance of temperature on the distribution of marine life. Some authors regard it as the sole important factor, others do not attribute any value to it at all. This diversity of opinion is less the result of a lack of facts, our store of which has been greatly increased by the last years' expeditions ; but it is rather to be attributed to an incorrect grouping of the facts. My intention, on the present occasion, is not so much to bring forward new data as to re-arrange those we already possess.

The conflict of opinion may be seen in the work of two investigators, whose particular line is geology, but who have occupied themselves especially with marine biology as a territory intermediate between the sciences of Zoology and Geology. I refer to Walther and Heilprin.

Walther says, at the end of a detailed argument, after enumerating many facts, which are to prove the influence of temperature, that the latter is not only *one* factor, but the exclusively determining one, in comparison to which all others, like light, atmospheric pressure, etc., are quite in the background.

Heilprin, in a discussion of the fauna of the deep sea, does not attribute so great an influence to temperature, and brings forward the following train of argument. Reef-building corals, which flourish best at 70–75° F., and which do not grow below 68°, would find this temperature in many oceanic parts, even at a considerable depth, in the Red Sea, for instance, even down to the bottom ; nevertheless, they occur everywhere only at the surface. Conversely, the fauna of the deep sea begins in the lesser depths (100 fathoms if there is a bottom), and, though the temperatures are much higher there than in the real abysses, the fauna is the same. The beginning of the fauna of the deep sea is always to be recognised at a certain depth, which is about the same in metres everywhere, but which must have a very

different temperature in the different seas and different latitudes. Moreover, if temperature is the determining factor, Polar animals ought to be the same as abyssal ones in lower latitudes, which is denied by Heilprin.

In the whole of this argument no clear distinction has been made between three classes of facts and relations :—

1. In the first place, no distinction has been made between pelagic animals, the Plankton and the Benthos, *i.e.*, the life confined to the shore or ground, animals either sessile or capable only of creeping, in contrast to the Nekton, the freely-swimming animals, capable of considerable movements, able to stem the tide and migrate at will.

2. Secondly, without further consideration a parallel has been drawn between the horizontal and the vertical distribution with reference to temperature, while the latter becomes complicated by other factors, too, as pressure, want of light, etc. Moreover, the gradations in the change of temperature in different depths are not all proportional to the amount of the depths themselves.

3. In the above argument no distinction has been made between eurythermal and stenothermal animals.

A few words ought to be said to explain these terms (first adopted by Moebius). We might give a definition by saying that eurythermal animals can bear great differences of temperature, stenothermal animals cannot, and this can be represented in a graphic manner. Each animal has a certain optimum of temperature, at which it flourishes best. A number of degrees above it its metabolism cannot go on ; and in a similar manner, at a certain degree below this optimum, the metabolism ceases also. Thus we obtain for each animal three points in the thermometrical scale—optimum, maximum, and minimum. Now in some animals the space on the thermometrical scale between maximum and minimum can be very great : these are eurythermal animals. In other cases it is small : these are the stenothermal animals.[1]

It is clear that eurythermal animals cannot be made use of either as a proof or a contradiction of the influence of temperature. Many animals of the shore, especially molluscs and echinoderms, are proved by experiments to be eurythermal. If a bivalve, for instance, is found both in the North Polar Sea and in the lower strata of the German or English Sea, nothing is proved *for* the influence of temperature—just as little as the occurrence of a eurythermal animal at the surface both in the Mediterranean and at Spitzbergen would be a proof *against* it. This has often been overlooked.

Moreover, it is easy to understand that it is necessary that the

[1] It is obvious that warm blooded animals can bear greater differences of temperature because they have their mechanism of heat regulation in their own body ; they are therefore *eurythermal.* The poikilothermal animals, on the contrary, to which the animals of the sea belong almost exclusively, are more stenothermal *à priori.*

animals of the Nekton, *i.e.*, the animals which swim well, should not be too sensible to temperature; for by their power of moving either in a horizontal or in a vertical direction, they pass easily into very different conditions and have to be adapted to them, if the faculty of swimming is to be of any use to them at all. As a matter of fact, we find that the large swimming animals, such as crustaceans and fishes, are, in general, eurythermal.

The geographical distribution of whales seems at first to contradict this consideration, but in reality it does not. For if we find some of their species chiefly inhabiting the high north or south latitudes, it is not because they are unable to endure higher temperatures, but because only in those regions can they find the enormous quantity of organic substance, of Plankton, which they require for their gigantic bodies.

Here we touch another point of importance. In high north and south latitudes, the production of Plankton, the primordial food, the algæ, the small crustacea, etc., is extraordinarily great, because the variations of temperature are here small, and therefore the life-conditions are very favourable, in spite of a low degree of temperature. This also has contributed to obscure the question of the influence of temperature. If an animal cannot exist below or above a certain degree of temperature, it is clear that the determination of the *average* temperature of a place is of small zoögeographical value; for this average temperature can just as well be the mean between two temperatures suitable for the animal as between two which are harmful or fatal to it. The attempt has therefore been made to draw lines of greatest cold as decisive for the question, and these are termed isocrymal lines by Dana. Not much, however, is gained by that; first, because we ought to regard the lines of greatest heat as well, and secondly, because it is not the degree of temperature alone, but the extent of variation which has a harmful influence. It is proved, in some instances, that the same animals can live at very different temperatures, at 3°, 7°, 15° Centigr., if only this temperature at the respective localities remains fairly constant to 3°, 7°, 15° Centigr.

These are eurythermal animals, but they are, nevertheless, stenoi (restricted), not with regard to temperature, but with regard to its variations.

I think a strange occurrence in marine life, upon which Semper laid stress, can be explained in this manner. Some Hexactinellids, typical sponges of the deep sea, occur in the Pacific Ocean, in very different temperatures; even at a comparatively small depth in tropical parts, in which the temperature is about 10° higher than in greater depths of the China Sea, in which they are to be found also. The factor which all these localities have in common, besides the absence of currents, is slight variation of temperature.

We have seen that the Nekton animals, being eurythermal, cannot be appealed to in *proof of anything* regarding the influence of

temperature; but the case is different with the two remaining biological groups of marine life, the Plankton and the Benthos.

The difference between the latter is the second point hitherto often neglected in the question of the influence of temperature.

By the term *Benthos* is meant any animal which passes its life in connection with the ground, whether it be the bottom of the deep water or the shore of the shallow water. We may distinguish, according to Haeckel, sessile Benthos, *i.e.*, animals totally fixed, and ambulatory Benthos, *i.e.*, animals capable only of creeping and crawling over rocks and sands, but not of swimming great distances at will like the Nekton.

Plankton is capable of travelling long distances, not at will, but only floating or drifting about with the currents. We might further distinguish a littoral Plankton, the Plankton of the shallow water, the shore, and the Plankton *par excellence*, that of the High Sea.

Distinct as these groups are, there may, nevertheless, be transitions between, even in the life of one single species. A Medusa, as fixed at the stem of a Polypoid, is a Benthos animal—when set free, an animal of the Plankton. But such cases occur only among the animals of the littoral Plankton, not in the Plankton of the high sea; there are other Medusæ which never have a sessile stage in their life, and are, like Salpæ, Radiolaria, etc., Plankton animals *par excellence.*

These different groups should not be confused with regard to their relations to temperature. Among the animals of the Benthos there are, as I have said, a number of eurythermal ones, as shown by experiment. But the majority are tuned, as it were, to a certain degree of temperature, which, just like the relations to the light, to the character of the bottom, and so on, must be reckoned among the peculiarities of the species. The Benthos animals are mostly stenothermal, and hence the influence of the temperature on them is very remarkable.

The coasts of the sea might, therefore, be divided, in the first instance, according to latitude or isocrymal lines, into *zones*, which are broken up into *areas* by a longitudinal division according to the continents.

Dana, Günther, and Agassiz have given us very valuable guidance for the animals of the shore. It is true that, by adding the longitudinal division to the zonary, *temperature*, which effects the arrangement into zones, does not remain the only factor in the distribution of the Benthos, as the facies of the bottom, etc., are not to be neglected. Still it is regarded as the most important one.

The distribution of the pelagic life of the open sea is not so clear. The longitudinal division is now without significance, because the influence of the continents does not come into the question. It would, however, be incorrect to think that the true Plankton animals are quite cosmopolitan.

From the *à priori* point of view, the warmth of the sun, different

in different latitudes, ought to regulate the distribution of real pelagic life; a system of belts would result parallel to the degrees of latitude; but this system of parallelism becomes disturbed by the action of currents and winds. On the other hand, by these currents sharper regions are defined; for, otherwise, all these zones would pass into each other gradually, which is not the case in reality.

Both north and south of the equator three circles of currents may be distinguished from 0–10, 10–50, 50–80° of latitude, and between them islands, as it were, of quiet water are formed, the so-called Halistases. These circles, *i.e.*, depending upon the temperature in them, constitute the most important factor in the distribution of pelagic life.

A current always shows differences of temperature at its outer limit, and by this circular arrangement the pelagic animals, as Brandt first laid stress upon, are kept within temperatures of a fairly constant degree; being always carried back to the circle-current of the temperature, and not being able to escape and flourish in the adjacent colder or much warmer water. This, as I wish to emphasise, is valid for the stenothermal animals. Eurythermal animals could be carried away to other regions, and continue to flourish there. Currents, therefore, act in two manners, either as helps to wandering, or as barriers to spreading.[2] In general, the latter will be the case for Plankton animals.

A division of the open sea into zones can, therefore, be accomplished. How great the difference can be on each side of a current may be seen chiefly at the Gulf—or Florida—Stream, as is illustrated by some instances noted by the Plankton Expedition.

While working out the Craspedota of this expedition, I could ascertain that no pelagic species of Medusa, occurring to the north of the Gulf Stream, is found also to the south of it, and *vice versâ*. The well-known *Aglantha digitalis* (described first by Forbes) has been caught constantly and in abundance from North England to Greenland. To the south of the Gulf Stream it ceases at once to occur. The same may be said of a Narcomedusa, *Solmaris multilobata*. On the other hand, *Aglaura hemistoma*, which has not been found in the north district, occurs with great regularity in the middle and equatorial part of the Atlantic, and the same can be said for the well-known *Rhopalonema velatum* and some species of *Liriope*.

Similar observations have been made in other groups of animals, for instance, in the pelagic annelids. The Tomopteridæ are abundant

[2] The occasionally abundant occurrence of animals generally rare or totally wanting at the British coast, can thus be explained. The animals in question are stenothermal animals of the Plankton. In ordinary years the Gulf Stream forms a sharp limit for them. If they are driven in the colder water by the current, they cannot grow there, but perish. Now, such uncommon occurrences have been recorded in years of uncommonly warm weather. Then the animals carried away from the south find a suitable temperature at the surface in the north, too, and the action of the current in such cases is dispersing.

in the north district, the Alciopidæ are wanting there, and occur only south of the Gulf Stream. For other groups, such as Appendicularia and Decapods hitherto worked out, the same sharp limit has been found. Most sharply defined limits are seen in the interesting pelagic copepod genus *Copilia*, which is wanting in the north district, but occurs not only abundantly but in many species in the equatorial part.

It is remarkable that, in the eastern district near Ascension, other species than those occurring in the same latitude near Brazil have been found. I have stated a similar fact for some Medusæ.

The explanation of this is rather simple in connection with the influence of temperature. Here a southern colder stream passes; stenothermal animals which are adapted to colder water find here a temperature that suits them, while they cannot grow and flourish in the tropical waters near Brazil. The influence of this same cold current has been stated to exist by Studer and by the "Challenger."

The family of Geryonid Medusæ in general, to which the genus *Liriope* belongs, are a very good example for the influence of temperature on Plankton animals, as Corals are for the Benthos. The Geryonids seem to require warm water and occur only at a certain latitude. They are abundant in the Mediterranean, but are wanting totally in the German Sea and the north district in general. They are abundant in the equatorial part of the Atlantic, and the same may be said of the Pacific. I have obtained them in great number from the warmer currents, which the "Albatross" expedition encountered. I also got them from the Red Sea and the Indian Ocean, but none from higher southern latitudes.

It is in harmony with this evident need of warmth that the Geryonidæ are typical animals of the surface. This is obvious from the so-called "Stufenfänge," *i.e.*, captures made at the same station in 50, 100, 200 m. depth; and they occurred even in the cylindrical surface-net of the Plankton expedition, where many other animals which had been found in 100 m., etc., were wanting. It is known by direct observation how their umbrellas project above the surface of the water, especially during contraction, so that Italian fishermen call them "capelletti di mare," or "little sea-caps."

From Agassiz's last three cruises there is the same result. A *Liriope* was the most abundant Medusa, always appearing at the surface, even when *Stomobrachium* and other species had retreated to a depth of 100–200 m., to avoid the influence of daylight and warm sun.

Here we arrive at the third and last point, the distinction between vertical and horizontal differences of temperature, between which a parallel has been drawn hitherto, but upon insufficient grounds. Down to the depth of 400 m. it is allowable to make a comparison between the changes in the life-conditions, especially temperature in different depths and the changes of the temperature of the surface in different latitudes; but in greater depth this becomes impossible, the light, the

atmospheric pressure becoming quite different. Even the temperature alone cannot be compared, for near the surface it diminishes very rapidly, and then more and more slowly until we reach the temperature of the abysses. Moreover, at a given time the temperature of a certain depth may be the same as that of the surface in a higher latitude, but the variations are quite different. In the Mediterranean the daily variations cease totally at 18 m., the seasonal at about 400 m., and as we have seen, the variations are much more important than the average temperature.

It was formerly concluded on theoretical grounds that there could be as well many vertical belts of different life as there are horizontal ones, but this comparison is, as I have shown, not correct or can only be carried to certain depths, not exceeding 400 m. This is not a theoretical conclusion only. All the unerring data, proving a relation between the diminishing of temperature in horizontal and vertical direction, are obtained in depths within the 200 m. limit.[3]

Considering the above facts, I do not think the horizontal distribution of the pelagic fauna is compensated by the vertical differences, so that pelagic animals would find the same temperature that they have near the surface in higher latitudes in a low latitude in greater depth. Vertical migrations cannot be so extensive (owing to the small power of locomotion of the Plankton animals); and in the greater depths conditions prevail which exist nowhere at the surface, in no latitude, and in no time of the year.

Somewhat striking are the observations of Chun, who found the surface animals of the winter in the Mediterranean at considerable depth in the summer. Setting aside the question as to whether his apparatus was adequate to prove with certainty the depth from which the animals are supposed to come, his observations can be explained by the influence of temperature.

In the Mediterranean the deep water is much warmer than in the Atlantic; the retreating animals would, therefore, not find such extremes of temperature as in the open ocean (which is, as Agassiz has stated, in some places, at least, totally destitute of pelagic life in such depths). In the Mediterranean, therefore, an intermediate pelagic fauna would be able to exist under the special life-conditions.

This is in full harmony with the investigations of the Prince of Monaco on the survival of the deep-sea animals of the Mediterranean. Formerly it was thought that the great atmospheric pressure is the most important factor in deep-sea life, and it had been concluded that the deep-sea animals coming to the surface would be killed principally by the great difference of the atmospheric pressure; but that has been refuted by the Prince. Atlantic animals

[3] *Echinocardium kurtzii* } occur in the Florida stream in a depth of 25 m., in North
Moïra atropos } Carolina at the surface.

Aeginopsis mediterranea { in the Mediterranean near the surface, in the tropical Atlantic in 200 m.

from 2,000 m. died when they came to the surface, while Mediterranean animals from 2,000 m. survived.

The Atlantic forms came from 0° to 20°, while those of the Mediterranean came from 13° to 20°. Hence it is obvious that temperature is here the most important factor.

In so far as real pelagic life is concerned, I therefore think that the existence of an intermediate fauna is not to be regarded as proved, neither in theory nor fact, and I would like to repeat Wyville Thomson's old sentence:—"The fauna of the deep water is confined to two belts, one near the surface, the other at the ground."

According to my opinion, the abyssal fauna is chiefly a Benthos fauna, more or less in connection with the bottom; a kind of *littoral* Plankton may be associated with it, but the true pelagic fauna does not exceed a certain depth.

I do not deny there is an intermediate zone of life, but it might be supposed to consist, scanty as it is, principally of Nekton, that is to say, of swimming animals, to which vertical distances are of no importance, and which are not sensitive to the changes of temperature. All of these are eurythermal, while, in contrast to them, most of the animals of the Benthos and Plankton are stenothermal.

In conclusion, after so much *theory*, I would merely repeat the *fact* that no case illustrates the great influence of temperature on the distribution of marine animals better than the well-known example of the Corals for the Benthos animals and the new one of Geryonid Medusæ for the Plankton.

Otto Maas.

V.

On the Dispersal of the Nutlets in certain Labiates.[1]

ONE of the commonest methods of seed dispersal in plants with capsular fruits is what is sometimes called the "censer mechanism." The seed-vessel is open at one end, and the seeds which lie loose at the bottom are thrown out by the swaying of the plant in the wind or by the disturbance of a passing animal. A familiar instance is afforded by the larkspur *Delphinium*, where the seeds collect at the bottoms of the follicles, and are easily scattered by shaking or striking the plant. Other instances are to be found in Hildebrand's well-known book on the dispersal of seeds (1). In some of the Labiates a condition of things biologically similar exists, but here the calyx plays the part of the censer, and it is the nutlets, not the seeds, that are shaken out.

In some cases arrangements are found which seem to render the dispersal more difficult, but it has been suggested that there is here a contingent advantage to the plant, because the period during which the seeds continue to be scattered is prolonged, besides which all the seeds are not likely to be thrown out in one direction, which would be the case if they fell out easily at the first waft of wind. The most usual arrangement is that the capsule stands upright on its stalk with a small opening at its upper end. Then, again, it often happens that the seeds are not free, but adherent to the placenta, so that a somewhat violent shock is needed to displace them. In the case of the Labiates it seems possible that the hairs which line the calyx in some species play the same part.

As a further refinement come those cases in which the capsule does not remain permanently open. Steinbrinck (2), Kronfeld (4), Beck (5), and others have described cases in which the mouth of the capsule closes in wet weather and opens again when the air is dry. A biologically parallel case has been noted by Rathay (3) in certain Compositæ where the involucre bends in and encloses the achenes in moist air. A similar state of things is described lower down among the Labiates.

The special advantage gained by these adaptations is not obvious. We have no evidence that the plant profits by the seeds or fruits

[1] Read at the Oxford meeting of the British Association, 1894.

being protected from wet, which is a possible end for such an adaptation.

Lastly come those cases of somewhat rare occurrence, in which the capsule or calyx, as the case may be, opens in damp air and shuts up when dried. These movements, with which others are coordinated, form the subject of the present paper. How far they are of biological importance to the plant it is not at present possible to say. It is only by the collection of a large number of instances that we can hope to arrive at a conclusion.

Hygroscopic movements, apparently adapted for the dispersal of seeds in wet weather, are confined chiefly, although not by any means entirely, to plants indigenous to countries having long periods of drought, during which the seeds, if dispersed, would have little chance of germinating.

Familiar instances of this phenomenon are found in the Crucifer, *Anastatica hierochuntina* (Rose of Jericho) and the Composite, *Asteriscus pygmæus* (Coss et Dur), both of which are inhabitants of deserts.

In 1878 Steinbrinck (2) drew attention to the fact that the capsules of two species of Veronica open more widely when sprinkled with water, and in 1883 he gave an account (6) of a similar movement in the capsules of *Caltha palustris* and several species of Mesembrianthemum and of Veronica. This was found by MacLeod (7) to hold true of *Veronica arvensis* and *V. serpyllifolia*. Volkens (8) and Schinz (9) have described other cases of hygroscopic movements of a like character.

Verschaffelt (10) has described the striking instances of *Salvia horminum*, *Brunella vulgaris*, *B. grandiflora*, and *Iberis umbellatum*. Finally, Ascherson (11) has given to these movements the name of hygrochasia; and, after reviewing critically the literature on the subject, he describes two new cases, *Lepidium spinosum* and *Ammi Visnaga*.

On examining about forty Labiates in fruit I have found fourteen species which show hygroscopic movements. Of these there are eleven species which move in such a way as apparently to favour the dispersal of their nuts in wet weather.

The method of observation was in each case as follows. The plant in the dry condition was drawn to scale. Water was then sprinkled on it, and when the consequent movements had taken place, a drawing was again made. In order to observe the more minute movements of the calyx-teeth, a single calyx was pinned down to a cork, under a microscope furnished with an eye-piece micrometer, and the change in the position of the teeth noted, when the calyx was wetted.

The movements may be classified in the following way :—

I.—The movements closely resemble those of *Brunella vulgaris* described by Verschaffelt (10).

The axis of the calyx is turned upwards so as to be almost vertical (the mouth being up), and the opening is more or less com-

pletely closed by the calyx-teeth. In rain the pedicels bend outwards until the axes of the calyces are nearly horizontal. At the same time the calyx-teeth bend out so that the mouth is open.

1. *Ziziphora capitata* (Linn.), Figs. 1–4.

A herbaceous annual, with erect stem. The flowers are borne in verticillasters, forming a dense terminal head. When in fruit, the four leaves immediately beneath the head are folded up round it, so as almost to enclose it. The calyces are almost erect, and the mouth quite closed by the teeth. The calyx is tubular, elongated, hairy outside, glabrous within, except for a thick ring of hairs at the throat. Rain or moisture cause a rapid movement to take place. The leaves folded up round the head bend out rapidly; the calyces bend outwards, so that they become more horizontal, and the calyx-teeth open. By sharply shaking the plant the nuts are thrown out to some distance.

EXPLANATION OF FIGURES.

Fig. 1. *Ziziphora capitata*; a capitulum in fruit; in the dry condition $\times 1\frac{1}{4}$ c·ca.)

Fig. 2. The same; wet condition.

Fig. 3. Calyx of *Z. capitata*; dry condition.

Fig. 4. The same; wet condition.

Fig. 5. *Ziziphora tenuior*, a spike in fruit; dry condition. ($\times 1$.)

Fig. 6. The same; wet condition.

Fig. 7. A small portion of the spike of *Z. tenuior*, most of the leaves and calyces having been removed; dry condition.

Fig. 8. The same; wet condition.

Fig. 9. The mouth of the calyx of *Z. tenuior* in the dry condition; longitudinal section, showing the ring of hairs in the throat and the teeth closed.

Fig. 10. The same; wet condition.

Fig. 11. *Lallemantia peltata*; a node of the spike, all the calyces and bracts having been removed except two; dry condition. ($\times 1\frac{1}{4}$.)

Fig. 12. The same; wet condition.

Fig. 13. Mouth of the calyx of *L. peltata*, seen from the front; dry condition, the teeth closing the opening.

Fig. 14. The same; wet condition; the teeth having moved slightly apart.

Fig. 15. *Ocimum campechianum* (?); part of the spike in fruit; dry condition. ($\times 1\frac{1}{2}$.)

Fig. 16. The same; wet condition.

Fig. 17. A calyx of *O. campechianum*; dry condition; the others in same whorl having been removed.

Fig. 18. The same; wet condition.

Fig. 19. The same; longitudinal section, showing the hairs at the throat.

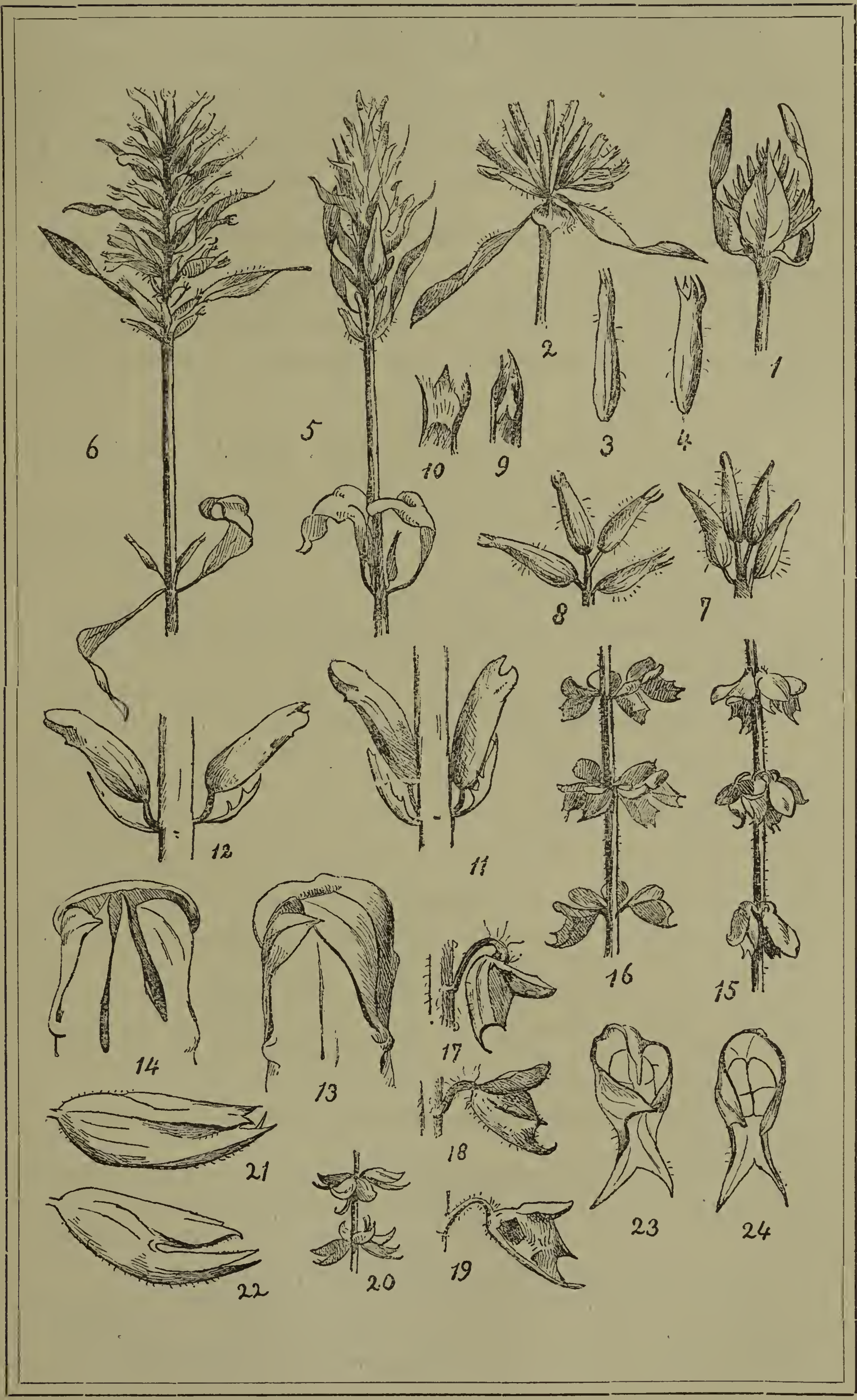
Fig. 20. Part of the spike of *Salvia gigantea*. ($\times 1$.)

Fig. 21. A calyx of *S. gigantea*; dry condition.

Fig. 22. The same; wet condition.

Fig. 23. *Lamium flexuosum*. A calyx seen from the front, showing the teeth curved across the opening; dry condition. ($\times 5$.)

Fig. 24. The same; the teeth bent apart; wet condition.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Habitat: Region of the Mediterranean and South Russia—fields and hills.

2. *Ziziphora tenuior* (Linn.), Figs. 5–10.

A herbaceous annual. The flowers are in few-flowered axillary verticillasters, forming a spike. The calyx is like that of *Z. capitata*, but more hairy externally. The movement is similar to that of *Z. capitata*. Both the floral leaves and calyces, which when dry are turned up round the main axis, bend out when wet away from it, and the calyx-teeth open.

Habitat: Region of the Mediterranean and South Russia—hills and uncultivated fields.

3. *Ziziphora hispanica* (Linn.). (Only a herbarium specimen of this was examined.) The stem is branching and herbaceous; the flowers grow in dense spikes. The movement is the same as in *Ziziphora capitata*.

Habitat: Spain.

4. *Ziziphora taurica* (Bieb. Fl. Taur. Cauc.). (Herbarium specimen.)

The flowers are in long leafy spikes; the movement the same as *Z. capitata*.

Habitat: Mount Taurus.

5. *Brunella hyssopifolia* (Lam. fl. fr.).

The movement is the same as that of *B. vulgaris* described by Verschaffelt.

Habitat: Region of Mediterranean Europe—open, dry places.

6. *Lallemantia peltata* (Fisch. et Mey.), Figs. 11–14.

An erect herb, with six-flowered verticillasters in the axil of sessile bracts. In fruit, the tubular calyces are nearly erect, and the bracts subtending them are folded up so as partially to enclose them. The pedicels of the calyces are flat and broad, being compressed in the median plane. The teeth are unequal, the posterior one being much the largest, flat and slightly curved forward over the four anterior teeth which almost close the mouth. In consequence of wet, the bracts and calyces bend down and outwards from the main stem until the axis of the calyx is at about an angle of 45 degs. At the same time, the four anterior calyx-teeth move slightly apart so that the opening is enlarged and the nuts are shaken out much more easily than when dry.

Habitat: Persia, Armenia, near the Euphrates, Kurdistan, Mount Elburz.

II.—The axis of the calyx points more or less downwards, and in consequence of wet it bends up until nearly horizontal. *Salvia horminum*, described by Verschaffelt, is the most marked example of this.

1. *Ocimum* [probably *campechianum*] (Mill), Figs. 15–19.

The stem is erect; the flowers are arranged in six-flowered verticillasters. The calyx when in fruit is bent downwards. The posterior tooth is broad and obtuse, with winged and decurrent

margins ; the other teeth are ovate ; the two lateral ones very shortly pointed, the two anterior ones more pointed. The mouth is open, but a ring of hairs closes the throat.

In consequence of wet the pedicel bends up so that the calyx becomes horizontal ; the two anterior teeth bend slightly down, and the two lateral teeth move a little apart, so that the opening of the mouth is slightly enlarged. The movements of the teeth can only be seen under the microscope.

Habitat : Central America.

2. *Melissa officinalis* (Linn.).

The stem is erect; the flowers are in lax, few-flowered axillary verticillasters. The calyx is pedicellate, two-lipped, slightly inflated, the mouth open. The upper lip is spreading, truncate, shortly three-toothed ; the lower lip is bifid, the two pointed teeth either pressed against each other or crossing each other. The throat is lined with a ring of hairs, which closes the mouth. The whole calyx is rather deflexed.

In consequence of wet the pedicels straighten, so that the calyces are raised to a nearly horizontal position. The anterior teeth are also lowered very slightly.

Habitat : S. Europe and M. Asia—woody hills.

3. *Salvia sclarea* (Linn.).

The flowers are arranged in six-flowered verticillasters. The calyx is shortly pedicellate, hairy, slightly bent downwards. The upper lip is tridentate, the lower bifid with acuminate teeth. In consequence of wet, the pedicels bend slightly up, so that the calyx is raised, and at the same time the teeth of the lower lip are very slightly lowered.

Habitat : S. Europe,—roads and dry places.

4. *Salvia gigantea* (virgata, ait. hort. kew.), Figs. 20–22.

The flowers are arranged in six-flowered verticillasters. The calyx is shortly pedicellate, campanulate, hairy. The teeth of the lower lip bend up so as nearly to close the mouth. The movement in consequence of wet is the same as in *Salvia sclarea*, but the downward movement of the lower lip is more marked.

Habitat : Italy.

III.—The movement is confined to the calyx-teeth.

Lamium flexuosum (Ten. fl. nap.), Figs. 23, 4.

The stem is erect ; the flowers are in dense axillary verticillasters. The calyx is bent downwards below the horizontal, subcampanulate, lined with short hairs. The mouth is partially closed by the teeth, the posterior tooth bending down forwards, and the two lateral ones bending across, so as to meet.

In consequence of wet the posterior tooth bends up until it is horizontal, and the lateral teeth bend out sideways, so that the nuts can be shaken out more easily.

Habitat : Liguria.

Besides the eleven species described above, there are three which also move hygroscopically, but in a different way.

These three, *Hyptis pectinata*, *Elsholtzia cristata*, and *Nepeta kohamerica*, have the axis of the calyx almost erect or else horizontal, and the mouth not closed except by hairs, and in consequence of wet the teeth close slightly. This movement is probably not connected with the dispersal of seeds, but with their protection from wet.

1. *Hyptis pectinata* (Poit. ann. Mus.).

The calyx is almost regular, campanulate, the mouth open, with spreading pointed teeth.

The flowers are arranged in cymes, which, when in fruit, are elongated, unilateral and pectinate, with the calyx turned vertically upwards.

In wet the peduncles bend up so as to bring the calyces near the main stem, and the calyx-teeth bend up so as to close the mouth.

Habitat: Equinoctial America—dry chalky and sandy places. Mexico, Tropical Africa, etc.

2. *Elsholtzia cristata* (Willd).

The flowers are in many-flowered verticillasters forming dense unilateral spikes. The calyx, in fruit, is regular, campanulate, the exterior hairy and glandular, the interior glabrous except for long hairs on the margin of the teeth which almost close the mouth. The axis of the calyx is horizontal or inclined upwards.

In consequence of wet, the calyx-teeth close together partially.

Habitat : Asia, and Europe introduced.

3. *Nepeta kohamerica* (Regel).

Stem erect branched. Flowers in terminal spikes. The calyx is hairy and tubular, with sub-oblique mouth, the calyx-teeth lanceolate. In consequence of wet, the calyx-teeth close slightly together.

Habitat : Plains of Kohamyr.

With the exception of the three last cases, the movements described above appear to favour the dispersal of the nuts in rain, yet it is not easy in every case to demonstrate this fact. In *Lallemantia*, the nuts are far more easily shaken out in the wet condition, but in *Ziziphora tenuior*, which has similar movements, this was not found to be the case. Possibly this may be due to the nuts not being ripe.

Salvia sclarea and *Melissa officinalis* have been placed with the others, but their movements are so slight that it is difficult to see how they can affect the dispersal of the nuts. In *Ocimum*, too, although the movement is very decided, yet the advantage gained is not evident.

In conclusion, I should like to express my thanks to Mr. F. Darwin, for having suggested that I should examine Labiates in regard ot the distribution of their seed, and also for his very kind help and criticism.

REFERENCES.

1. **Hildebrand.**—" Verbreitungsmittel der Samen."

2. **Steinbrinck, C.**—" Untersuchungen über das Aufspringen einiger trockenen Pericarpien." *Bot. Zeitung*, xxxvi., 1878.

3. **Rathay.**—" Austrochnungs und Imbibitionserscheinungen bei Cynareen Involucren." *Sitzb. d. k. Akad. Wiss.* Wien, 1881.

4. **Kronfeld.**—" Verbreitungsmittel d. Compositenfrüchte." *Sitzb. d. k. Akad. Wiss.* Wien, 1885.

5. **Beck, G.**—" Untersuchungen über den Oeffnungsmechanismus der Porenkapseln." *Verhandl. Zool. Bot. Ges.* Wien, 1885.

6. **Steinbrinck, C.**—" Ueber einige Fruchtgehäuse die ihre Samen infolge von Benetzung freilegen." *Ber. d. deut. bot. Ges.* Berlin, 1883.

7. **MacLeod, J.**—" Veronica arvensis und V. serpyllifolia." *Bot. Jaarboek*, 1887.

8. **Volkens.**—" Die Flora d. ægypt. arab. Wüste," 1887.

9. **Schinz.**—*Abh. Bot. Ver. Brandenb.*, xxxi., 1889, p. 186.

10. **Verschaffelt.**—1. " Die Verbr. d. Samen bei *Brunella vulgaris*, *B. grandiflora*, *Salvia horminum*, *S lanceolata*." *Bot. Jaarboek*, 1890.

    2. " Die Verbr. d. Samen *Iberis amara* u. *I. umbellatum*." *Bot. Jaarboek*, 1891.

11. **Ascherson, P.**—" Hygrochasie und zwei neue Fälle dieser Erscheinung." *Ber. d. deutsch. bot. Gesellsh.*, x., 1892.

DOROTHEA F. M. PERTZ.

VI.

Hertwig's "Preformation or New Formation."[1]

Part III.

DR. HERTWIG entitles the concluding part of his paper, "Thoughts Tending to a Theory of Development of Organisms." He professes, not to work out a theory in detail, but to indicate the direction in which guidance is to be obtained. Let it be remembered that in his criticism of Weismann's preformationist views there were two main points. First, he thinks not only that Weismann has failed to prove the existence of "heirs-unequal division," but that there are many grounds for supposing that all cell-division is a case of "heirs-equal" division. Hence there is no reason to suppose that the various elements of the germ-plasm are marshalled to their places in the course of an individual development, and that this sorting out of preformed determinants is the reason why an egg produces with imperative fidelity the thousand different cells of the adult. Every cell of the whole organism, so far as inherited dower goes, is identical. Next, Hertwig throws doubt upon the whole conception of determinants, pointing out that very many of the individual characters of organs and of organisms are not properties of cells at all, but are properties of aggregates of cells, and therefore cannot come into existence until aggregates arise, and certainly can have no preformed determinant within a single cell.

Hertwig is disposed to steer a middle course between preformation and epigenesis, to cull from either theory what seems reasonable and in consonance with observed fact, and to reject much of the speculation with which both preformationists and epigenesists have rounded off their conception of Nature. As yet, we are far from having a complete explanation of the problems, but it seems to him as if Nature were a follower neither of Weismann nor of his opponents, but evenly pursued a middle way.

It is necessary to suppose that each organism starts with a specific dower. However much can be attributed at present, or may come to be attributed, to the moulding forces of environment, there

[1] Zeit- und Streitfragen der Biologie. By Professor Dr. Oscar Hertwig. Pamphlet I. Präeformation oder Epigenese? Grundzüge einer Entwicklungstheorie der Organismen. Pp. 144, with 4 illustrations in the text. Jena: Gustav Fischer, 1894. Price 3 marks.

remains a large residuum, which can be explained only by the transmission of a specific plasm from the parent to the sexual cell. In the same tank of sea-water, surrounded by the same baneful and useful circumstances, there may lie two fertilised egg-cells not differing appreciably in appearance, in size, in chemical composition, or in physical characters. Yet one of these produces a sea-hedgehog, another a worm. The resemblances between the eggs of species of the same genus may be still more remarkable, and the distinctive characters may not appear till a late period in the course of the individual development; but the different specific characters do appear, although there is nothing in the environment to cause them. The egg of a barn-door fowl is as fully stamped with the character of its species as is the adult animal.

To explain the process of development, Hertwig believes that we must postulate the existence of a material which contains germs or incipia of a particulate character, and which possesses an extraordinarily complicated structure corresponding to the marvellously minute and complicated different ways in which it must react, as the growing organism is built up by cell-multiplication. This material differs from Weismann's conception of germ-plasm in many ways. First, it is not built up of determinants and so forth into a complicated architecture. Hertwig makes no supposition whatever as to the physical nature of his specific material. Next, it by no means contains all the determining causes of the organism. Many of these are due to external causes: many more to the reaction between external causes and the material itself. In this respect, it is true, there is no vital difference between the two theories. Weismann at first attributed too much to the germ-plasm and overlooked a number of ways in which the environment plays a direct part in ontogeny. But recently he has reviewed and accepted many external causes which he had hitherto passed by. Both Weismann and Hertwig regard this specific material as containing the residual factors of development. Weismann may understate, or perhaps Hertwig overstate, the forces which have to be allowed for before the residuum is reached; but both are agreed that there is a necessary residuum for which a specific inherited material must be assumed. We come now, however, to the great point in which the two views are wide as the poles asunder. Weismann, as everybody knows, attributes to the sexual cells, and to special regions called germ-tracks, the exclusive possession of the germinal plasm, making an exception only of a few cases of special and probably secondary adaptations. In consequence, he draws a sharp distinction between the cells which contain the plasm and the somatic cells. New organisms arise only from cells containing germ-plasm, and the somatic cells, when they become specialised, lose the power of reproducing the species as they contain, not germ-plasm, but a simpler derivative of it. Hertwig, on the other hand, totally denies this sharp distinction. All the cells in the body receive an equal dower of the specific

material, because in cell-division there is always an equal division. No doubt sexual cells, for the most part, arise only in special parts of the organism. But, in these cases, the fact that certain regions of the body give rise to the sexual organs is merely a part of the general ordering of the body into tissues and organs, and is not due to the presence in sexual cells of any special plasm. Hence it is not surprising to find in special cases diffused powers of reproduction or of regeneration, any more than it is surprising to find muscle-cells or gland-cells in unwonted situations.

In one matter, the possibility of the inheritance of acquired characters, a question which bulks more largely in popular controversy than in Weismann's theory, the two views do not differ theoretically. Weismann admits the possibility of the influence of outside forces modifying the germ-plasm in the cells, and as Hertwig's specific material is diffused through all the cells of the body, it too is not removed from the possibility of being altered by the environment. Hertwig, however, does not specially deal with the controversy as it is known, but one draws the inference from the general tendency of his views, that his specific material is supposed to have as much organic stability as the germ-plasm.

The epigenetic part of Hertwig's views about development consists of an account of many of the factors, apart from a specific plasm, which direct the development. The leading motive in his account of these is one diametrically opposed to Weismann's view that a cell must become what it is, and develops only in accordance with the determinants which it received from among the determinants in the parent cell from which it arose. Hertwig believes that a cell becomes whatever it is made to become by the forces round about it. The limit to this is given by the specific material with which it is endowed. A cell of one animal could not be made into the cell of another, but the cell of an animal might have been any other cell of the animal had its environment been different. In the words of Driesch, the specialisation of a cell is a "function of the locality of the cell in the organism." The causes of the elaboration of an organism from its germs or incipia lie in conditions outside the incipia in the egg-cell, but which, nevertheless, follow in regular and orderly sequence. The first of these causes is the continual change in the relations of cells to each other and to the external world as the organism is built up by cell-multiplication. Physiologically speaking, the differentiation of cells in different directions is due to the reaction of the organic substance to different external stimuli. The capacity of the cell to assimilate or to go through metabolic changes in the case of the fertilised egg-cell produces the cleavages by which the morula is formed. The particles of the protoplasm were arranged round the nucleus of the egg-cell as a single centre of forces. In the morula they are arranged round a number of new centres corresponding to the new nuclei. Thus, even in equal-heirs

division, differences arise in the cells according to their position and relations to each other, and the cells are different qualitatively from the original cell from which they sprang.

In many respects the form assumed by the cell-masses is a function of organic growth. The original cell, from the physical nature of the protoplasm, naturally has a spherical shape, and the retaining of this shape by the cell-mass produces differences in the shapes and surfaces of the constituent cells. The next important factor is the necessity that growth shall take place so as to secure the greatest possible extension of surface in proportion to mass. A crystal grows by the deposition of new particles upon the outside; organic matter grows by the intersusception of matter from the different constituents in the environment, and it is therefore necessary that as large a surface as possible shall be exposed to the environment in order that the varied substances may be picked up.

The organic material in process of growth can assume only those forms which allow it to remain in touch with the outer world. A cell-mass could not grow indefinitely thick without the inner cells being deprived by the outer cells of their relations to the environment. For this reason, cell-multiplication produces threads and layers and membranes, rather than solid masses. In the plant kingdom the cells, for the most part, are strengthened by a firm cell-wall, through which their liquid and gaseous food is able to pass. The possession of this cell-wall enables plant growth to remain, for the most part, in the form of threads and membranes, and the necessary extension of surface is produced in the simplest possible way. In the adult plant all the organs and tissues grow out so as to preserve this simple extension of surface. In the animal kingdom the method of nutrition by the absorption of solid masses into the interior of the cells prevents the formation of thick cell-walls. Animal growth, then, cannot rely upon the nature of the individual cells to secure the necessary rigidity and coherence of the whole mass. The cell-masses grow so as to form hollow bladders and tubes, the whole structures possessing a rigidity not found in the individual cells, and growth takes place by the infolding of the single layers of cells which form the walls of the spheres and tubes. In plant-tissues the food-supply comes from the water and the air of the outer world, and in consequence the extension of surface is as much as possible an extension turned to face the outer world. In animals the solid food is taken into an internal cavity, where it is broken up and ingested, and in consequence there is as great as possible an extension of this inner surface. Thus, the blastula is a hollow sphere, the walls of which consist of a single layer of cells which retain contact with the environment on their inner and outer surfaces. An inpushing of the wall of the blastula gives rise to the gastrula, and subsequent infoldings in the inner and outer walls of that give rise to muscle layers, nerve layers, cœlomic pouches, and so forth. But in all these

changes the cells in different places find themselves in very different circumstances; those lining tubes and hollows inside the body are brought in contact with very different external forces from those lining the outer side of the body. In correspondence with these varied stimuli the organic material, the same in all the cells, responds in different ways, producing cell-specialisation.

In the epigenetic modification the next process which helps to produce the specialisation of the organic body is of a physiological nature. Every cell is an elementary organism, endowed with the capacities of elementary organisms. But the cells resulting from the division of the egg to a certain extent lose their individuality in the new individuality of the whole. The purely morphological method of regarding cells tends to exaggerate their individual importance. Although the whole shape and form of the organism is produced by cell-multiplication, yet, considered as an organism, the multicellular adult is better regarded as a continuous mass of an organic material. Hertwig details many of the facts showing the intimate correlations which exist between different parts of organisms, and concludes that many of the elaborations of development must be set down to the observed fact of correlative growth. This part of his argument is perhaps more conclusive in so far as it is a criticism of Weismann's doctrine of the existence of associated determinant to account for correlations, than it is in itself satisfactory. Weismann may have failed completely in giving a plausible explanation, or he may have underrated the importance of correlation as a factor in growth. Hertwig simply associates the two observed facts of the physiological unity of organisms and the existence of correlated growths.

He elaborates at some length his view that different external influences cause the development of identical incipia into different final results. In course of this, he reviews the controversy between Spencer and Weismann relating to the polymorphism of social insects, and concludes with Spencer against Weismann that Natural Selection is not the factor at work, but that the different environments of the different forms have produced the divergent modifications. He sees in it the same phenomenon as the production of roots from stems, or of the upper part of a hydroid from the cut lower part. It is one of the results of the identity of the organic matter all through the cells and tissues of an individual, and of the capacity of this identical matter to respond in varied ways to varied stimuli.

The three great points of his theory are these. First, all organisms possess a specific plasm of exceedingly complicated nature. Secondly, the sexual cell, which is the origin of a new organism, possesses a share of this plasm, and, being an elementary organism, is able to grow and multiply. In this process of multiplication, the resulting cells, formed as they are by equal-heired division, receive each an unmodified share of the initial plasm, which, like the cells

containing it, increases in bulk by growth. Thirdly, the specialisation and differentiation of the organs and tissues is due to the action of varied external stimuli upon the same organic substance. In his own words :—

" My theory may be called *evolutionistic*, because it assumes the existence of a specific and highly-organised incipial plasm as the basis of the process of development. It may be called *epigenetic*, because the incipia grow and elaborate from stage to stage only in the presence of numerous external conditions and stimuli beginning with the metabolic processes preceding the first cleavage of the egg-cell, until the final product of the development is as different from the first incipia as adult animals and plants are from their constituent cells."

P. CHALMERS MITCHELL.

SOME NEW BOOKS.

A Living "Palæontology."

Éléments de Paléontologie. By Félix Bernard. Seconde Partie. 8vo. Pp. 529–1168 and i.–viii. Paris: Baillère & Sons, 1895 (August, 1894). Price 15 francs; price of the complete work 25 francs.

We expressed our appreciation of the first part of this book in April of last year (Natural Science, vol. ii., pp. 307–308), and after carefully examining this second part, which deals with Lamellibranchs, Cephalopods, Vertebrates, and Plants, we do not feel called upon to modify the opinions previously given. In many respects this text-book of palæontology is superior to any other with which we are acquainted, and we should hesitate to say that it is in any respect inferior to those others. Its virtues are its own; its vices are common to all similar works. It is, therefore, more useful as well as more pleasant to praise the former than it is to point out the latter.

Dr. Bernard's chief merit is that he treats extinct animals from the standpoint of the biologist, laying stress on such as are of morphological importance while passing lightly over those genera and species that have acquired a fictitious value as the tokens or date-counters of the stratigraphical geologist. Evolutionist as well as biologist, he bases his classification in each group on its ascertained phylogeny, and remembers that in this difficult study evidence must be collected from the palæontologist, anatomist, and embryologist alike. Thus every fact that finds its place in this book is of a definite value, and is quoted for a definite purpose. Joined by the thread of scientific theory, such facts are easily retained in the memory of the student, and the work, instead of being a dry cram-book or catalogue, assumes the form of an interesting narrative, where the deepest truth proves the highest art. Since we can never have the conclusion of the lamented Neumayr's "Die Stämme des Thierreiches," we are grateful to Dr. Bernard for so soon applying the methods of that distinguished writer to a working text-book for students.

Turning to the execution of this volume, it is plain that Dr. Bernard has taken much trouble to get up the recent literature, and has spared no pains to discuss all views of any importance. Thus, over two pages are devoted to an account of the Ammonite aptychus and of the various explanations of it, while the different theories as to the origin of the Horse are admirably summarised. The errors that mark the work of a compiler, as distinct from that of an original investigator, are, however, easily to be found. To ascribe to Smith Woodward the views quoted on p. 800 as to the crocodilian skull does no great harm; it merely shows that the author has obtained his knowledge of fossil crocodiles from Mr. Woodward's semi-popular summary rather than from the original

sources. Similarly, though Dr. Bernard makes a point of ascribing his figures only to their original authors, we note some curiously erroneous ascriptions; for instance, fig. 402 is not by Lydekker but by David Page, while Nicholson should hardly be credited with the well-known figure of the teeth of *Cochliodus* by Agassiz. To take one more instance, no man that had ever examined a *Sepia* shell for himself could have called the "pad" the homologue of the pro-ostracum of the Belemnite, or could have denied that the posterior end was septate. Lapses such as these will, however, always and inevitably be found in the work of those writers who, in Transatlantic phrase, bite more than they can chew. The critic is obliged to point them out, and when they rise above a certain percentage he is obliged to be severe, though some offending authors are of the contrary opinion; and possibly the authors are not wholly wrong in their protest, for after all the people that really deserve chastisement are those who encourage young and untried writers to grapple with a subject that demands the co-operation of numerous specialists. The authors are whipping-boys for these, the true offenders, so let the strokes be lightly laid on!

To detail the treatment of the various groups dealt with in this second section would be lengthy and wearisome. We shall only venture on a few remarks.

The account of the Cephalopoda is clearly influenced by Munier-Chalmas, a fact by no means to be regretted, for that naturalist, though he has undoubtedly thought much, has published little on this fascinating subject. Hence we find the order Ammonoidea removed from the Nautiloidea, and boldly placed alongside of the Decapoda and Octopoda. We admit that very much may be said in favour of this association; but it is possible to go too far. A short time ago Mr. H. Woods informed the world that the Ammonoidea possessed "two pairs of gills, two pairs of auricles, and two pairs of kidneys." Now Dr. Bernard states that they are "pourvus de 2 branchies et de 2 oreillettes," and that they have a "Système nerveux à ganglions bien délimités." It is clear that these gentlemen cannot both be right; and, as neither of them has as yet offered an atom of proof in support of his assertions, it is perfectly open to us to maintain that neither of them is right. Indeed, Dr. Bernard admits that if we knew the animals that inhabited the Ammonite shells we should probably attach less importance to names derived from the number of gills. Why continue to assume a knowledge though you have it not? Owen's classification was excellent and the obvious one, so long as attention was fixed on recent Cephalopoda; but the researches of Branco, Hyatt, Munier-Chalmas and others on extinct Cephalopoda have made its retention, even in a modified form, a conspicuous absurdity.

To the families that constituted the Dibranchiata of Owen the name "Belemnoïdes" is here applied, a name that has always struck us as singularly unhappy when thus extended to Cephalopods unprovided with a belemnon or guard and even in some cases without a shell. The account of this order is well up-to-date, but we regret still to find statements that we believe incapable of proof. *Spirula*, for instance, is surely not the "Belemnoid" most nearly allied to the Nautiloid and Ammonoid type. It is coiled in the inverse direction, while the statement that its body is for a considerable period contained within the shell is a pure assumption. It is probably descended, as Dr. Bernard admits, from *Spirulirostra*, and is therefore separated from the coiled Ammonite shell by all its

ancestors at least as far back as the Trias. We believe that the Belemnoteuthidæ form the starting point of the Chondrophora rather than of the Phragmophora or Osteophora; but the question admits of argument. Neither can we agree with Dr. Bernard when he states that without doubt the Tetrabranchs have given birth to the Dibranchs, or when he maintains that some Goniatites are directly derived from some coiled Nautiloids, or when he writes that the Chondrophora are manifestly descended from Belopteridæ and Sepiadæ.

In the pages allotted to Vertebrata, the author hardly acts up to his principles in devoting so small a space to the Amphibia or "Batraciens." The extinct Stegocephali possess a peculiar interest as vying with the Theromorpha for the honour of Mammalian ancestry. Dr. Bernard indeed inclines to derive Mammals from the Theriodont Reptiles, but there is much to be said in favour of their direct descent from Amphibia. It is not so much the peculiar skeletal characters, such as the two occipital condyles, the single temporal arcade, the bony symphysis of ischium and pubis, that are in the way of their descent from Reptiles, for these characters were present in the lowest Reptiles as well as in the highest Stegocephali. But it certainly seems easier to derive the Mammalia from the ancient Amphibia with primitive vertebral column, anteriorly directed ilium, five-fingered hand and glandular skin, than it does to derive them from any such forms as had advanced in the direction of the Reptilian type. At any rate, these are questions not yet settled, and for a more complete understanding of them the student desires a better account of the Amphibia than is given by Dr. Bernard, as well as more reference to the writings of E. D. Cope.

Passing over the remaining chapters on Vertebrata, we need merely mention that the dentition of the Mammalia is very clearly described, with figures and diagrams explanatory of the numerous terms now employed. This part concludes with a fuller account of ancient man than is usually found in works of this class and size.

Having devoted 800 pages to the animal kingdom, our author proceeds to compress the whole of Vegetable Palæontology into 87 pages, which 87 pages we have little hesitation in saying are simply wasted. No one that wishes to study the subject is likely to come here for his information, while the zoologist will only grumble at the additional size of the book. The classification employed in this part would have been the better for a little revision. The essay on the phylogeny of plants pays scant attention to the views of anyone but the Marquis de Saporta. No mention is made of Treub's work on *Casuarina*, or more recent work in Russia and in this country on some of the Cupiliferæ, all of which has a very important bearing on phylogeny. A mistaken sense of duty, probably on the part of the publishers, was no doubt the cause of the insertion of this inexact and insufficient chapter.

The book closes with a few additional notes, some errata, and an excellent index. We note that the author did us the honour to read our former remarks, though he does not seem to have fully understood them.

Till better methods enable a more accurate text-book of palæontology to be compiled, we recommend this to the notice of students. It is always interesting, often original, yet judicial, well illustrated, and on the whole very good value for a sovereign.

F. A. B.

THE PHYSICAL GEOLOGY AND GEOGRAPHY OF GREAT BRITAIN. By the late Sir Andrew C. Ramsay, LL.D., F.R.S. Sixth Edition, edited by Horace B. Woodward, F.G.S., of the Geological Survey. London: Edward Stanford, 1894.

A SCIENTIFIC book which goes through five editions in fifteen years has, we may be sure, either something very attractive about it, or contains a store of information for which there is a steady or growing demand, or has the good luck to combine both claims to popularity. That the last was the case with the late Sir A. Ramsay's "Physical Geology and Geography of Great Britain," all those who know the book, and this includes everyone who is interested in its subject, will be prompt to admit. It is not merely that it gave a summary, condensed but singularly clear, of the bare facts, stratigraphical and palæontological, connected with the geology of our islands; there are many who possess the knowledge that would enable them to do this perhaps equally well. But these dry bones were made to live; clothed, as it were, with flesh and muscle and organised tissue, when it was shown how intimate was the connection between them and the scenery, the climate, the industry, and the racial peculiarities of our native land. A further charm lay in the enunciation of sundry theoretical views, due largely or wholly to the brilliant ingenuity of the author, many of which hold their own, or have even been strengthened by the advance of knowledge, while some are still matters of dispute. And all was done in such a way that the book was a vivid reflection of the individuality of the author. Those who had the good fortune to know him intimately, had brought back to them the genial talk which was the echo of his strong convictions, and the enthusiastic emphasis, tinged and heightened by a steady glow of humour, with which he insisted on the doctrines that he held to so tenaciously. Time has weakened but little, for the writer of these words, the remembrance of those "dies noctesque deorum"; and for him, and many another, no lapse of time can weaken the admiration and love for the master that grew out of them.

But other things are more transitory. Editions become exhausted, to the profit, we may hope, both of publisher and author. The growth of knowledge demands, in the new edition, corrections and additions, maybe omissions, to bring it up to date. With the majority of books the revision presents no great difficulty, and requires no more than sound judgment and an acquaintance with the latest phase of the science they treat of. In the case of such a book as that now before us, there must be besides the sympathy which enables the reviser to feel wherein lies its individuality, and the tact which will put right what is in obvious need of modification without impairing the special flavour of the work.

It is gratifying to find that these points have been kept in view by the editor of the sixth edition. Without attempting a minute collation of it, and the edition that preceded it, we are convinced that every attempt has been made to place the book on the level of the knowledge of to-day. A sketch is given of the various views held as to the British pre-Cambrian rocks. *Olenellus* is figured; the discovery of *Conocoryphe viola* is recorded; notices of Overthrust faults, of Mr. G. Barrow's paper on Themo-metamorphism, and of the occurrence of Radiolarian Chert in the Glenkin Shales and Mullien Island are inserted. The researches of Mr. H. T. Brown on the Permians of Leicestershire, and of Mr. Clement Reid on the Cromer

Section, receive due recognition. These examples, picked at random, are types of what has been done to keep pace with the march of geological research.

Without wishing to be over critical, one of his points may be noticed where there is still perhaps room for improvement. It is hard to see why, in the table on page 400, the Devonian should be sandwiched in between the Upper and Lower Old Red Sandstone. In putting forward the conjecture that stratified rocks may possibly be liquefied by intense metamorphism (p. 36), a word of caution might have been usefully added. Surely Dura Den is not the locality for *Anodonta jukesii* and *Palæopteris* (p. 94); nor can there be any doubt now as to the age of the Reptiliferous Sandstone of Elgin (p. 128). The episodal character of the Corallian Beds is hardly brought out with sufficient distinctness; and in the question of nomenclature between Neocomian and Lower Cretaceous there is something like a halting between two opinions; Glauconitic would have been better than Chloritic Marl. In the case of the Upper and Middle Eocene, the time has come to make their representatives in Hampshire and the Isle of Wight, about which our knowledge is fairly complete, the type, in place of the Bagshots of the London Basin, about which we know (*pace* Dr. Irving) scarcely anything. In the case of the Oligocene, more might have been said about the great earth movements which culminated towards the end of that epoch. Some objection may be taken to what is said about the shell-bearing gravels of Macclesfield. It is hardly possible for anyone who has seen these beds in section and collected their fossils to believe they can have been driven up by an ice-sheet from an adjoining sea bottom. The perfect state of many of the shells may be accounted for by supposing that they were frozen into the matrix during their journey. But anyone who has noted the bedding and current-bedding of the gravel can hardly fail to be convinced that they are subaqueous deposits laid down where we now find them. That the shells indicate different depths of water is natural, for the gravels were probably formed during subsidence and an increasing depth of the sea.

The later chapters, though judiciously modified, are happily allowed to retain much that gave to the book from the first its distinctive character. Lastly, by omissions, none of any serious moment, and rearrangements, often resulting in manifest improvement, the book has been reduced from 639 to 421 pages.

A. H. Green.

The Royal Natural History. Edited by Richard Lydekker, B.A., F.R.S. London: Frederick Warne & Co., 1894. In monthly parts. Each, price one shilling nett.

Ten numbers of this interesting popular Natural History have now been published, and the public, Mr. Lydekker, and Messrs. Warne and Company must be congratulated. The get-up of the volume is all one could wish for, and the illustrations, of which some are taken from Brehm's well-known "Thierleben," while others are specially prepared for this work, are a great improvement on anything that has been published before. The coloured pictures, on the whole, we do not care for, though some of them are well done. The first two parts of the work are devoted to the monkeys, the third to the lemurs and bats, the fourth to the hedgehogs, moles, and cats, which latter continue into the fifth, and are succeeded by the genets, civets, and

Bears Walking.
(From Lydekker's "Royal Natural History.")

mongooses. In the sixth part are fossil hyænas, dogs, and foxes; in the seventh, bears, raccoons, and weasels; in the eighth, seals and their kind, and we are introduced to the oxen, which continue in part 9, and are succeeded by the sheep, goats, and antelopes. Part 10 carries us well into the deer. Mr. Lydekker's descriptions are distinctively and pleasantly written, and much interesting and anecdotal matter is incorporated. Special attention too has been paid to the occurrence of each form, when known, in the fossil state.

Taken as a whole, we do not know of any work that may be so satisfactorily recommended to those who wish an acquaintance with the mammalia. An additional interest will be found in the living animals at the Zoological Gardens if studied family by family by the aid of Mr. Lydekker's Natural History.

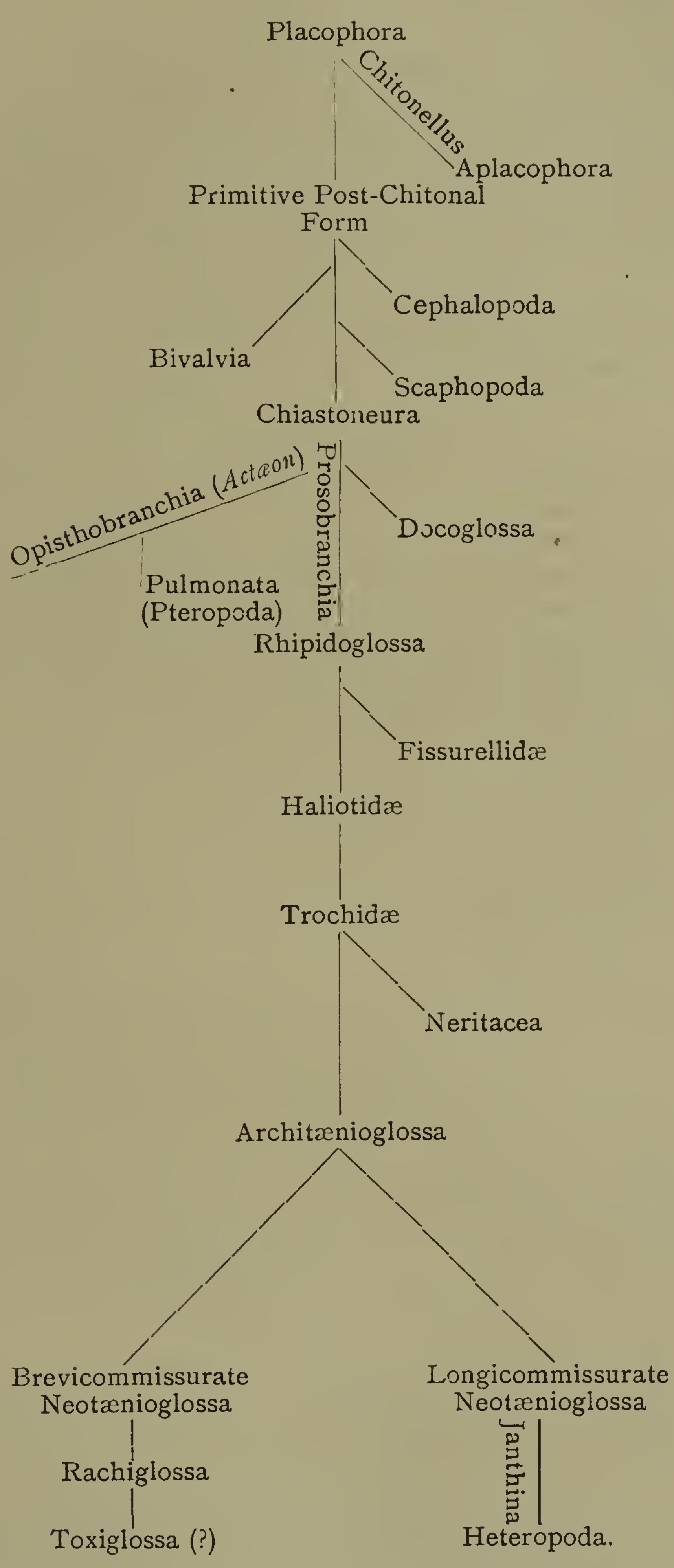
Limpets and their Allies.

Studien über Docoglosse und Rhipidoglosse Prosobranchier nebst bemerkungen über die phyletischen Beziehungender Mollusken untereinander. By Dr. B. Haller. 4to. Pp. iv., 173; 12 plates, coloured. Leipzig: Engelmann, 1894.

The greater part of this interesting and most important work is taken up with the description of the anatomy and morphology of the Docoglossa, or Limpets, and the Rhipidoglossa, or group to which the Keyhole-Limpets, the Ormers, and the Topshells belong. The Docoglossa are divided by the author into Bibranchia, comprising the two-gilled *Propilidium*, Monobranchia, or those with the single neck gill, and the Cyclobranchia, or those, like the common Limpet, which have a circlet of gills. Where the Lepetidæ, which have neither the neck-gill nor gill-circlet, come in is not apparent. Of these three groups, the first named is considered to be the oldest, the others following in their order. Of the Rhipidoglossa, the genus *Pleurotomaria* is held to be the oldest, though Dr. Haller speaks of its anatomy being quite unknown, having apparently entirely overlooked Dall's account of it in the Bulletin of the Museum of Comparative Zoology at Harvard College, xviii. (1889), pp. 397–403, a circumstance not altogether to be wondered at considering the way in which the eminent American naturalist buries his best results in papers whose titles give no clue to their valuable contents.

Dr. Haller's last twenty pages are, however, the most interesting from a general point of view, containing as they do his conclusions concerning the phylogenetic relations of the various members of the Molluscan stem. These conclusions are of great importance, and will be best made clear by reproducing his own diagram.

On contrasting this with the tree drawn up by Pelseneer (Natural Science, iii., p. 37), which is now very widely accepted, certain prominent differences, apart from mode of drawing up, become apparent. Thus, to begin with, Dr. Haller takes the Chitons as his base, and derives all the others from it, including the Cephalopoda, although both their embryology and geological history are against such a supposition. Other equally startling conclusions, such as the classing of the Pteropoda with the Pulmonata, the sea-butterflies with land-snails, may be observed, and further comments can be safely left to the reader. Whether Dr. Haller's tree will come unscathed through the fire of criticism is doubtful, but every thoughtful attempt to unravel the genetic relationships of a group is of extreme value, and we are proportionately grateful to the author for this latest contribution to Molluscan literature.


Placophora
Chitonellus
Aplacophora
Primitive Post-Chitonal Form
Cephalopoda
Bivalvia
Scaphopoda
Chiastoneura
Opisthobranchia (Actæon)
Prosobranchia
Docoglossa
Pulmonata
(Pteropoda)
Rhipidoglossa
Fissurellidæ
Haliotidæ
Trochidæ
Neritacea
Architænioglossa
Brevicommissurate Neotænioglossa
Longicommissurate Neotænioglossa
Rachiglossa
Janthina
Toxiglossa (?)
Heteropoda.

The Salmon of the Elbe.

Der Elbelachs: eine biologisch-anatomische Studie. By Professor Dr. Anton Fritsch. 8vo. Pp. 116, with 84 figs. and 1 coloured plate. Prague: F. Rivnac, 1894.

In the midst of numerous other occupations during the past twenty-five years, Dr. Anton Fritsch has been engaged in collecting information concerning the salmon and salmon-fisheries of the Elbe. So long ago as 1871 he published some preliminary observations on the subject, followed later by other notes; and now he has done good service by summarising the results of his long-continued research in the little profusely illustrated volume before us. The first part of the work is devoted to a sketch of the life and migrations of the salmon; the second and third parts deal respectively with economics and the anatomy of the fish; and a few remarks are appended in reference to the future of the salmon-fishery of the Elbe. The volume is issued with the aid of a grant from the Bohemian Parliament, and will prove of great value to those concerned in the industry of which it treats.

The account of the salmon fisheries, as might be expected, occupies the greater part of the work; but the chapter on anatomy is by no means meagre, and the descriptive details are illustrated with numerous drawings by the author. There are also sketches of external features which vary at different periods, and a few illustrations of the principal parasites known to infest the fish.

The Organisms of Ponds and Rock Pools.

Ponds and Rock Pools: with Hints on Collecting for, and the Management of the Micro-aquarium. By Henry Scherren. Small 8vo. Pp. 208, with numerous woodcuts. London: Religious Tract Society, 1894. Price 2s. 6d.

This attractive little volume comprises a series of articles recently contributed by Mr. Scherren to the *Leisure Hour*. It is adapted as well for the general reader as for the beginner, and differs from most works of its kind in being much more than a second-hand compilation. The author is an enthusiast in his subject, and has himself verified most of the facts he records; while all the hints as to collecting and the management of the aquarium are the result of wide personal experience. It is, in short, a noteworthy book, not only to be recommended as a guide to the amateur, but one likely to infuse enthusiasm in a youth who has previously had little opportunity of looking into the subject of which it treats.

Mr. Scherren's hints on "pond and rock-pool hunting" are, as we have said, thoroughly practical. He writes not only for the wealthy pedant, who can afford elaborate apparatus, but remembers also the humble school-boy who is obliged to make his own simple tackle. The introductory chapter on these matters occupies over thirty pages, and has nine illustrations. The second chapter deals with "the beginnings of life," and emphasises more clearly than usual the difference between unicellular and multicelluar animals. The remarks on classification are also surprisingly up-to-date for a popular work. There are good figures of the *Amœba*, *Vorticella*, and other typical Protozoa. Chapter III. treats of sponges, hydrozoa, etc., and is illustrated by several original figures, chiefly of marine

animals. The common *Hydra*, as usual, merits a full share of attention, and we are pleased to observe some reference to the correction of the commonly-accepted errors resulting from Trembley's experiments on this animal. The worms follow, and are described in the words of Huxley as a "heterogeneous mob." Rotifers, of course, are treated at considerable length, and there are good figures—some original, some after Hudson. The Polyzoa are also very well described. Chapter V. relates to the larvæ and more minute forms of starfishes, arthropods, and molluscs; but this would bear much expansion and further illustration with advantage.

The concluding chapter on the micro-aquarium will be found as useful as that on collecting, and interest is added to it by a reproduction of the original engraving of Trembley's study, showing the glass jars in which the well-known experiments on *Hydra* were made. A view of the author's arrangements forms the frontispiece. A good index is appended.

A History of the World.

Histoire du Monde: son Evolution et sa Civilisation. By Etienne Guyard. 8vo. Pp. 700, with Schrader's Planisphere. Paris: published by Author, 5, Impasse Nicole, 1894. Price 7 fr.

This is a somewhat strange work by a former professor in the Imperial Military Academy of Japan. It is the first volume of a projected series of five, intended to be a popular account of the astronomical and geological history of the globe itself, an outline of the evolution of its animals and plants, and a general synopsis of the history of man from primæval times to the conditions of modern political and social life. The volume before us treats of astronomy, botany, zoology, geology, anthropology, and the history of the progress of organic evolution, and bears evidence of having been most laboriously compiled from the best modern sources. So far as we have been able to test them, the statements are generally clear and reliable; while to those who desire only a superficial knowledge of the multifarious subjects treated, and do not object to a somewhat bald, encyclopædic style, the work will doubtless prove itself very useful. The great inconvenience in the book, in our opinion, is, that the author has adopted the Oriental custom of placing the title page at what we term the end, while the pagination is exactly the reverse of the ordinary. Moreover, there is a great dearth of illustrations. The appended maps, however, are very good, and the bulk of the volume would doubtless be too much increased if every section were illustrated in the manner customary in special treatises.

Plant Physiology.

Experimental Plant Physiology. By Dr. Walter Oels. Translated and edited by D. T. Macdougal, University of Minnesota. 8vo. Pp. 86, with 77 illustrations. Minnesota: Morris & Wilson, 1894.

This English edition of Dr. Oel's admirable little book will prove a useful guide to a series of elementary experiments in illustration of the most important principles of plant-life. The many clear and excellent illustrations form a valuable supplement to the descriptions. The contents are arranged under the following headings: derivation of nutriment from soil and water; transpiration; photosynthesis (a term suggested by Professor Conway Macmillan to express

the decomposition of carbon dioxide and water by chlorophyll in the presence of sunlight); respiration and metabolism; geotropism; heliotropism; warmth; growth; movement; relation of plants to animals. The last-named is treated in a very perfunctory manner, only three simple experiments being suggested, whereas it is a subject which offers an almost infinite scope, and is, moreover, of the highest interest. The book is nicely printed, though misprints are not rare, and sentences are occasionally a little involved. We think, too, it is unnecessary to speak of sodium chloride as cooking-salt in connection with plant nutrition. The apparatus required for the various experiments is fairly simple and will be found in any moderately well-equipped high school or college laboratory. As is suggested in the introduction, good plant material is an important part of the equipment, and it is therefore essential that every laboratory undertaking the work should have ready access to a greenhouse or small botanical garden.

The Carboniferous Insects of Commentry (Allier), France.

The Coal-measures of Commentry, Central France, have in recent years, under the skilful exploration of their director and engineer, M. Henry Fayol, yielded a valuable collection of fossils illustrating the fauna and flora of the Upper Carboniferous period.

The specimens were placed in the hands of specialists; the plant remains were delivered over to MM. Renault and Zeiller, who have already published the results of their examinations, so likewise the fishes, by Dr. Emile Sauvage, and the insects were entrusted to M. Chas. Brongniart, of the Natural History Museum, Paris.

M. Brongniart has now completed his share of the work, and has embodied his results in a monograph embracing Neuroptera, Orthoptera, and Homoptera.

The work forms a volume of about 450 pages in 4to, and an atlas of 37 plates in folio.

Sixty new genera and 100 new species are described.

The work is accompanied by a detailed examination of the nervation of the living Neuroptera, Orthoptera, and Fulgoridæ.

The value of this publication, and its interest to palæontologists and entomologists tracing out ancestral forms cannot be overstated, as it gives an insight into an insect fauna rich beyond compare, and proves that those "forests primæval" were neither tenantless nor voiceless.

Our knowledge of palæozoic insects has been hitherto scanty, owing to the rarity of specimens and the fragmentary condition, for the most part, in which their fossilised remains have been found. This has all been changed by the remarkable deposit at Commentry, which has yielded ten times more specimens than all other localities in Europe and America conjoined. Not only are the specimens more abundant, but they are found in a state of entirety and preservation most advantageous for study—a fact no doubt due to the fine matrix in which they are embedded and the conditions under which they were entombed.

Mr. S. H. Scudder, the eminent American entomologist (in *Amer. Journ. Sci.*, Feb., 1894), writing on these insects and M. Brongniart's drawings, which he has recently had the opportunity of examining in Paris (facts which I can confirm from my own inspection of them two or three years ago), says: "I have had the opportunity, through

the kindness of M. Brongniart, of seeing not only a considerable part of this collection, but also the illustrations prepared by M. Brongniart himself from the choicest specimens; illustrations made with a care and exactitude which leave nothing to be desired, and which are now nearly completed after a labour of ten years, so that we may hope soon to be favoured with his final work. Leaving the cockroaches out of account, to which M. Brongniart will give his attention later, the number of these illustrations, their variety, the extraordinary character of the insects themselves, and their rare perfection, leave not the least room for doubt that when his work appears, our knowledge of palæozoic insects will have been increased three- or fourfold at a single stroke, and an entirely new point of departure for the future opened. No former contribution in this field can in any way compare with it, nor even all former contributions taken together. Besides, it will offer such a striking series of strange forms as cannot fail to awaken the attention of the least incurious. One may not enter into details, but mention may simply be made of one species, regarded by M. Brongniart as one of the forerunners of the dragon flies, in which the wings have an expanse of considerably more than two feet (or about 70 centimetres), and of which several specimens are preserved. It is a veritable giant among insects."

MARK STIRRUP.

L'ANTHROPOLOGIE. Paris: vol. v., nos. 1, 2, 3. 1894.

THERE are several papers of geological interest in these three parts of "L'Anthropologie." Emil Cartailhac describes the flint implements which have been secured from L'Herm Cave, Ariège, and Marcellin Boule has a note on the lower jaw of *Gulo luscus*, and in a similar bone of an enormous *Felis* from the same cavern. This second jaw, which measures 295 mm., is larger than any yet recorded, and M. Boule names it *F. leo*, race *spelæa*. Ed. Piette writes some notes illustrative of primitive art of the horse and reindeer periods. Good illustrations accompany the paper, one of especial interest showing an auroch's head cut into a piece of stone. Another article by Salomon Reinach treats of European primitive sculpture showing Græco-Roman influence, but this, though of extreme interest, deals more or less with historic peoples.

IN a recent paper entitled "The Mammalia of the Deep River Beds" (*Trans. Amer. Phil. Soc.*, vol. xvii.), Professor Scott makes some valuable additions to our knowledge of the American Equidæ, and describes some forms of which only brief notices had previously been given. Of these, Desmathippus is notable since it fills almost the only important gap left in the phylogenetic history of the horse, coming midway between Miohippus and Protohippus. The teeth are short-crowned with the valleys partly filled by cement; the feet possess fairly well-developed lateral digits. Another interesting form is a true Anchitherium, *A. equinum*, nearly allied to the European *A. aurelianense*, but in some respects more horse-like. The author shows that these are good reasons for regarding Anchitherium as a side branch of the equine phylum which has left no descendants, and considers that the genus must have arisen in America, probably from Miohippus. *A. equinum* occurs in the upper division of Deep River Beds which is taken by the author as approximately equivalent in age

to the Miocene of Sansan, a somewhat lower horizon than that to which it has previously been referred.

The paper also includes descriptions of the Carnivora, Rodentia, and Artiodactyla from the same deposits.

We have received a "Geological Sketch Map of Western Australia, 1894, by Harry Page Woodward, Government Geologist, Perth." The Recent and Tertiary rocks form a fringe of varying width along nearly the whole of the coast, and are somewhat extensively developed in the southern portion, or Eúcla division. The Mesozoic rocks, bounded on the west by the Recent and Tertiary, and on the east by the Metamorphic and Palæozoic rocks, extend from near Gingin, north of Perth, northwards to Cape North-West. The Palæozoic rocks (Carboniferous and Devonian) are largely developed; they have an enormous extent in the neighbourhood of the Gascoyne, Henry, Ashburton, and Fortescue rivers, and also in the Kimberley division; they appear also on the Irwin river and to the north of Albany. The Metamorphic and Igneous rocks are shown to occupy a much greater area than even the Palæozoic; they stretch through the south-west and Gascoyne divisions as a broad band, nearly parallel to the western coast, and occur also in the north-west and Kimberley divisions. The various gold-fields are indicated, as well as the districts in which copper, lead, tin, and coal have respectively been found. This map should prove of the greatest value to all who are interested in the geology of Western Australia.

We may add that three other works on Western Australia are also reviewed in the September number of the *Geological Magazine*. It is rapidly taking a foremost place among the Australian colonies, and in extent of territory it greatly exceeds the others. A sum of £71,482 has lately been voted for public works and buildings, including £2,000 for the building of a museum in Perth.

In the first part of the new *Proceedings of the Victoria Institute of Trinidad*, Mr. R. J. L. Guppy gives an account of the edible molluscs of Trinidad. Two or three kinds of oysters are eaten by Europeans, but among the lower orders they are in little favour. *Asaphis deflorata*, when well cooked, Mr. Guppy pronounces most delicious; *Mytilus brasiliensis* and *Venus granulata* are common in the markets. The North American clam is a *Venus*, but is a very different species, and is absent in Trinidad. *Donax*, and that large bivalve *Pinna*, are commonly used, but Mr. Guppy has no personal experience of them. Nearly all the fresh-water molluscs are eaten. The land molluscs are less in request, although the large *Bulimus oblongus* is said by French writers to have flesh which, though somewhat leathery, possesses a delightful aromatic flavour. What Mr. Guppy believes to be the kitchen middens of the aboriginal inhabitants show that a larger number of molluscs were consumed by them.

Mr. R. J. Lechmere Guppy sends us a reprint of his paper on the Microzoa of the Tertiary and other rocks of Trinidad and the West Indies. The paper appeared in the *Journal of the Trinidad Field Naturalists' Club*. It sums up the results of former publications by the

same author, and adds many new observations. It is accompanied by a table showing the distribution of foraminifera in the "Cretaceo-Tertiary" rocks of Trinidad.

We congratulate the botanical seminar of Nebraska University on the work achieved in connection with the flora of the State during last year, as indicated in the Report for 1893. The additions number 182, and bring the total number of species reported up to 2,820. Mr. Clements describes several new species of Fungi, and new hosts are recorded for fifteen others. It would be well if some separation of the great groups were indicated; as it stands, Algæ, Fungi, Mosses, and flowering plants run on in one unbroken series. But the great fault lies in the unsparing use of a trinomial nomenclature; if a plant is a variety let it be so stated—the practice of stringing names together only leads us back to pre-Linnean times. The pamphlet also contains a revision of the nomenclature of the Nebraska Polypetalæ, according to the latest ideas; *Bursa bursa-pastoris* does, however, call forth a protest from the author, Mr. P. A. Rydberg. Mr. F. E. Clements supplies a list of botanical expeditions in Nebraska from 1803 up to date, and Mr. R. Pound a bibliography of the flora of the State.

The Orchid Review (August) suggests that in the hybrid Disas, of which we have seen and heard so much lately, we may get a race of easily cultivated common greenhouse orchids. They grow like weeds, only requiring suitable compost, plenty of water, a little shade, and protection from frost. They will certainly be a beautiful addition to the list of plants within the reach of the amateur. The great centre for Disas is at the Cape, whence they run up into tropical Africa, occurring as far north as the mountains of Abyssinia, and also in Madagascar.

Die Natürlichen Pflanzenfamilien, under Professor Engler's direction, progresses slowly. The last issue, a double one, containing nos. 106 and 107, completes section 6A of part iii., and includes the remainder of Cactaceæ, by K. Schumann, and Geissolomaceæ, Penæaceæ, Oliniaceæ, Thymelæaceæ, and Alæagnaceæ, by E. Gilg. Also a portion of Borraginaceæ, by M. Gürke, belonging to part iv.

A *résumé* of the Geology of Spitzbergen is being published in the *Feuille des jeunes Naturalistes*, by M. Gustave Dollfus. The writer gives a geological map after Nathorst, and reviews the previous writings on the geology of the island. The Tertiary, Jurassic, Triassic, Permo-Carboniferous, Devonian, Silurian (?), and Archæan rocks are represented, and various volcanic rocks have been recorded.

IN the *Feuille des jeunes Naturalistes* for September, M. G. de Rocquigny has an interesting note on the pairing of butterflies of different genera. The forms noted were a male of *Satyrus janira*, and a female of *Vanessa urtica*. During the flight *S. janira* was carried by *V. urtica* and hung lifeless (*inerte*). The observation was made at Allier, on June 23, 9.45 a.m.

We have received from R. Friedlaender & Sohn, of Carlstrasse, Berlin, nos. 9 to 13 of their fortnightly publication. It professes to give a list of all new books and of the numbers of periodicals as they are issued which deal with Natural Science. It seems to us a publication to be commended to all scientific persons, and may be had post free for the modest annual subscription of four shillings.

Yet one more local scientific publication. *Madagascaria* is a monthly, dealing with the zoology, geology, botany, and anthropology of Madagascar, which Mr. F. Sikora intends to produce, starting on October 1, at Antananarivo, a place that as yet has not got beyond its well-known *Annual*. Yearly subscriptions of six shillings and fourpence (eight francs) may be sent to the Comptoir National d'Escompte in Paris, addressed, "Pour Mr. Sikora à Antananarivo."

OBITUARY.

FRANÇOIS CLÉMENT MAILLOT.

Born 1804. Died 1894.

SO commonplace is now the employment of quinine in the treatment of malarial fevers that the very names of those who first advocated its use are already omitted from our text-books, and in danger of passing from our memories.

The death of Dr. Maillot, at the advanced age of 91, recalls the services he rendered sixty years ago, not only to medicine, but to the world at large. Educated at Metz, he entered the career of military medicine, and in 1834 he was stationed at Bona, in Algeria, then newly colonised by France. The malarious climate was producing an enormous mortality among the new-comers, and it seemed an open question whether it would be possible to retain the colony. Maillot, who had already had some experience of malaria in Corsica, at once devoted himself to the study of the intermittent fevers of the district and their treatment by large doses of quinine, and with such success that the mortality from malaria is said to have fallen from 25 to 5 per cent. The actual discovery of the value of the cinchona alkaloids in the treatment of ague was, of course, long before this; but it may at least be claimed for Maillot that his advocacy of the employment of quinine not only popularised the remedy and saved thousands of lives, but demonstrated also the possibility of European colonisation of a highly malarious country.

A tardy recognition of his services to military medicine was made in 1888, when a pension of 6,000 fr. was conferred upon him; but it was not till last year that his numerous writings, dating from 1834 to 1887, and all devoted to this one subject, were collected and published in connected form.

REV. W. M. HIND, LL.D.

Born 1815. Died 1894.

WE regret to announce the death of the Rev. W. M. Hind, LL.D., Rector of Honington, Suffolk, at the ripe age of 79. For a long period he had devoted the greater part of his spare time to botanical field work, and was possessed of a wide and accurate knowledge of our British wild plants, many of the rarer forms of which he had cultivated and observed. Born in Belfast of English

parents, and educated at the Academy, he passed to Trinity College, Dublin, where he graduated B.A. 1839, M.A., 1842. In 1876 he edited an enlarged and revised edition of J. Cosmo Melvill's "Flora of Harrow," towards the original edition of which he had very largely contributed when a curate at Harrow. In 1875 he accepted the Chancellor's living of Honington, Suffolk, and immediately set to work to compile a flora of Suffolk, which he published in 1889. Having collected a large Herbarium of Suffolk plants, they were presented to the Ipswich and East Suffolk Museum. In 1870 he presented a very large and almost complete collection of British plants to the Trinity College, Dublin, Museum, and as an acknowledgment of the gift, was presented with the honorary degrees of LL.B., LL.D. by a special grace of the Senate. Dr. Hind was a man of high intellectual attainments and wide and varied reading. Essentially of a scientific mind, and an ardent and enthusiastic worker in his own subject, his wide botanical knowledge was always at the service of others.

The death is announced of Admiral Sir Edward Augustus Inglefield, K.C.B., who commanded three Arctic Expeditions, the first in 1852 in search of Sir John Franklin, and added much to our knowledge of the geography of the Arctic Regions. He had reached the advanced age of 74 years.

We have also to record the deaths of Pierre Louis Jouy, who had studied the ornithology of Japan, Korea, S. Arizona and Mexico; Dr. Erich Haase, Director of the Royal Siamese Museum in Bangkok, and a special student of the Arthropoda; Dr. Adolph Hannover, of Copenhagen, a well-known Anatomist and Histologist; Professor Michele Lessona, President of the Academy of Sciences and Director of the Zoological Museum in Turin; the nonagenarian, Dr. Louis de Coulon, one of the founders and, for most of its existence, the President of the Society of the Natural Sciences in Neufchatel.

The death of Professor Hermann von Helmholtz, on September 9, deprives Physiological Science of one of its most brilliant investigators. We hope to give some account of his work next month.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Dr. J. Playfair McMurrich has resigned his position in the University of Cincinnati, to accept the Professorship of Anatomy in the University of Michigan, at Ann Arbor.

Several botanical appointments have recently been made. Mr. T. H. Kearney, jun., succeeds the late Dr. Moring as Curator of the Herbarium of Columbia College, New York. Dr. W. Scott has been appointed Director of Forests and of the Botanical Garden in Mauritius. Dr. A. Zimmermann becomes Extraordinary Professor of Botany in the University of Tübingen; and Dr. Solereder is now a Curator at the Munich Botanical Institute.

We regret to learn of the retirement of Dr. E. P. Ramsay from the Directorship of the Australian Museum, Sydney. Dr. Ramsay has been in failing heath for some time, and the duties of his office have fallen upon Mr. Robert Etheridge, jun., who has now been chosen as successor. Mr. Etheridge entered the Australian Museum as Palæontologist in 1887 on leaving the British Museum, and will assume his new office on January 1, 1895.

At the Geological Congress at Zurich it was decided, upon the instigation of Captain Marshall Hall, supported by Professor F. A. Forel, to appoint an International Committee for furtherance and record of observations upon existing glaciers in different parts of the world. We will give further information so soon as the committee is constituted.

The Superintendent of the Indian Museum, Calcutta (Surgeon-Captain A. Alcock), has issued an admirable "Guide to the Zoological Collections exhibited in the Invertebrate Gallery of the Indian Museum." It is a little volume of 155 pages, and forms a most concise introduction to Invertebrate Zoology.

The Bristol Museum is closed for internal and external decoration, and is now being put into a thorough state of repair. Wires have been introduced for electric lighting, the dilapidated cases have been repaired, and the long-neglected collections will thus soon be again safely housed. The advent of municipal control has effected all these improvements, and it would be well if a few more of our languishing provincial museums could be similarly transferred to their respective towns. Mr. Edward Wilson, the energetic Curator, has just issued a new edition of his popular penny Guide, which continues to have a large sale.

M. Stanislas Meunier has inaugurated an annual exhibition of geological specimens at the Museum of the Jardin des Plantes, Paris, for the purpose of assisting geologists to follow and verify the work of the year. The exhibition is to be

not merely national, but to contain contributions from the various foreign Geological Surveys. To illustrate the past year the Geological Survey of Great Britain has lent specimens of the Archæan and pre-Cambrian rocks of North-west Scotland, geological maps in course of preparation, and a collection of new geological works and photographs. Signor Pellati, the Director of the Geological Commission at Rome, has sent specimens illustrative of recent Italian researches. The United States and Queensland Surveys have sent copies of their publications; and the Imperial University of St. Petersburg has lent a variety of specimens of platiniferous rocks, the discovery of which is of great importance. A small guide-book of 24 pages is issued in connection with the exhibition.

Important changes are taking place in the Palæontological Department of the U.S. National Museum at Washington. The Honorary Curator of this Department is Mr. Walcott, the new head of the Geological Survey. Till last July there were five honorary curators, who, though they advanced the collections, had little time to spare for purely curatorial work. Now, however, Mr. Charles Schuchert has been appointed Assistant Curator, with a salary, and is able to devote his time to practical improvements which will doubtless bring the exhibition series of this Department to the high level attained in the rest of the Museum. For the present, exhibition will be limited to a good representation of the North American fossil faunas geologically arranged, but not as yet, owing to want of space, divided into geographical provinces. The invertebrates will be displayed in the table-cases, the plants and vertebrates in the wall-cases. As soon as possible Mr. Schuchert will begin a synoptic collection illustrative of all fossil genera, in which each class will begin with a series explaining the terminology, after the fashion of the Crinoids in the Geological Department of the British Museum.

In addition to much information that has already appeared in our pages, the *Annual Report of the Delegates of the* [Oxford] *University Museum* for 1893 contains the following items of interest. In the Ethnographical Department of the Pitt-Rivers Museum series have been arranged which illustrate Magic, the early development of Writing, Lighting, Smoking, Treatment of the Dead, and Primitive Pottery. As evidence of original research conducted in his department, the Professor of Physiology quotes a list of six papers by five authors. Similarly, the Professor of Comparative Anatomy enumerates three ladies and six gentlemen who have used his senior laboratory for original investigations. The Professor of Zoology states that Dr. F. A. Dixey has studied the Pieridæ in the Hope Collections, in the preparation of an important memoir upon the evolution of the wing-markings in this group of Lepidoptera; the specimens have at the same time been arranged in a single series. Colonel Swinhoe has continued his Catalogue of Oriental Heterocera in the Hope Collections, and the second volume, containing the Pyrales and the Noctuæ, will soon be issued. The Hope Professor is breaking the tenth commandment in respect to the Mathematical Professors, whose room is better than their company. The delegates concur. The fittings of the new geological laboratory are all but complete, and advanced teaching can now be carried on in comfort, and with greater thoroughness than heretofore. A rock collection for teaching purposes is being systematically arranged and illustrated by explanatory labels. We fully agree with the Professor of Geology when he says, "Re-arrangement of the fossils is urgently needed, and it is hoped that it may be begun shortly. It will be a long and heavy piece of work, and skilled assistance will be absolutely necessary to carry it out successfully." The Professor of Mineralogy is taking in hand the re-arrangement of the mineral collection; this, too, not before it was wanted.

By the provisions of the will of the late Dr. William Johnson Walker, two prizes are annually offered by the Boston Society of Natural History for the best

memoirs written in the English language on subjects proposed by a committee appointed by the Council.

For the best memoir presented a prize of sixty dollars may be awarded; if, however, the memoir be one of marked merit, the amount may be increased to one hundred dollars at the discretion of the committee. For the next best memoir, a prize not exceeding fifty dollars may be awarded. Prizes will not be awarded unless the memoirs presented are of adequate merit. The competition for these prizes is not restricted, but open to all.

Each memoir must be accompanied by a sealed envelope enclosing the author's name and superscribed with a motto corresponding to one borne by the manuscript, and must be in the hands of the Secretary on or before April 1 of the year for which the prize is offered.

The Subjects for 1895 are:—

(1) A study of the "Fall line" in New Jersey.
(2) A study of the Devonian formation of the Ohio basin.
(3) Relations of the order Plantaginaceæ.
(4) Experimental investigations in morphology or embryology.

The Subjects for 1896 are:

(1) A study of the area of schistose or foliated rocks in the eastern United States.
(2) A study of the development of river valleys in some considerable area or folded or faulted Appalachian structure in Pennsylvania, Virginia, or Tennessee.
(3) An experimental study of the effects of close-fertilisation in the case of some plant of short cycle.
(4) Contributions to our knowledge of the general morphology or the general physiology of any animal except man.

In all cases the memoirs are to be based on a considerable body of original work, as well as on a general review of the literature of the subject.

CORRESPONDENCE.

Plateau Man at the British Association.

In reading over the report of the British Association discussion on Professor Rupert Jones's paper in the last number of Natural Science,[1] I am struck with a series of curious coincidences, and as I consider they are calculated to propagate a very wrong impression, I venture to ask to be allowed to point them out. I do not for a moment suppose it was intentionally done, especially in the face of the opening sentences, still it is very strange that almost every adverse criticism raised by the speakers is given, while nearly all arguments for, and admission of, the antiquity of plateau man are overlooked, and a summing-up given which may not always be taken in the way which a further examination of the facts of the case would demand. This is all the more to be regretted, as every supposed argument urged against plateau man rises on the speakers not being in possession of all the facts of the case. If it were simply an instance of various interpretations being put upon well-known phenomena of the origin of which we know but little, differences of opinion are all that could be expected; but in this subject—as indeed is elsewhere suggested in reference to all others—the value of one's opinion is not to be estimated alone by qualifications in other branches of science, however nearly related they may be to the question at issue, but by the amount of careful, extensive, and prolonged research in and out of the field he has given to it, qualified by his ability to observe. I am very much tempted to answer the criticisms raised at the British Association meetings one by one, but for my confidence in the effect of the publication of the results of recent labours. Those who are in possession of the facts of the subject have but one opinion; those who differ from us will alter theirs as knowledge accumulates. Truth is doubtless as precious to them as to ourselves, although some people are apt at times to forget that change of opinion is an honest man's characteristic. I thus hasten to a further examination of the discussion. Now I think the way this can best be done is to go through the remarks of each speaker, in so far as they bear upon the point at issue. The first speaker was Mr. Whitaker, in whose speech I find the following :—"This certainly carries man back, locally at all events, beyond the time of the river gravels, which occur in the bottoms and along the slopes of the valleys." He appears (*Times* report) to have been followed by Mr. Montgomerie Bell, who said that, in his opinion, "they (the plateau relics) belonged to pre-Glacial or Pliocene times." The next speaker was Sir John Evans, who admitted "that the principal outcome of recent discoveries was, to his mind, the fact that the existence of Palæolithic man could be carried further back in time than the valley gravels, inasmuch as flints are found in gravels on the plateaux at far higher levels." Sir John was followed by Dr. Hicks—who has had far too much experience in this subject to deny the pre-Glacial existence of man in Britain,—and he pointed out the great concession that had been made by Sir John Evans, and also how the same had been wrung out of the veteran Professor Prestwich by the repeated discoveries of man's work in pre-Boulder Clay deposits. Then followed Professor Boyd Dawkins, who made some sweeping assertions, which, like the adverse criticisms of other speakers, can be flatly challenged; but so far as the question at issue is concerned, he said he agreed with what Mr. Whitaker and Sir John Evans had said, and made no exception to the extension of time they had granted. General Pitt

[1] Reprinted in full in the present number, pp. 269-275.—Ed.

Rivers, from another standpoint, argued the *necessity* of pre-Palæolithic man. Sir Henry Howorth gave it as his opinion that the only evidence available for testing the relative age of the plateaux gravels goes to show that they are older than the distribution of the so-called Glacial Drift. With reference to Mr. Clement Reid's remarks, I am pleased to say he is not informed up-to-date; there are numerous localities now in various parts of the kingdom which have produced these relics of greater antiquity. Among the latest I might mention the work of Dr. Blackmore in his own neighbourhood, and the yields of the hill-drifts in Essex; the Boulder Clay of Finchley, and of deposits older than these in Middlesex, Essex, Suffolk, and Norfolk. The theory suggested by the concluding speaker, variously reported as Lt.-Col. Godwin and Col. Godwin Austin, calls for the existence of huge glaciers in the Weald, which would certainly take us back very much further in time than many anthropologists have been in the habit of admitting for man.

I cannot help thinking that a perusal of these facts will better emphasise your opening remarks and give a truer idea of the state of scientific opinion of to-day upon this point, and show that the verdict of the discussion is that opinions are divided, but that the affirmatives are in excess of the negatives; this in proportion to the greater the knowledge of the subject possessed by the respective speakers. It must also be admitted that it was quite lamentable to observe upon how little practical knowledge of the deposits the adverse criticisms were based; one speaker, at least, still confounding them with "the Clay-with-flints."

W. J. Lewis Abbott.

The Museums' Association.

The note headed "Science at a Picnic," in your issue of August, is so entirely misleading, so far as it regards the Museums' Association, that we ask permission to offer some corrections, especially as the note has evidently been written by some one not present throughout the meetings. The meeting commenced on Tuesday, June 26, with visits to various scientific institutions, lasting four hours, and finishing with the President's address at night, the whole time occupied being five and a half hours. On Wednesday three and a half hours were devoted to papers and discussions, and another three hours to the inspection and discussion of the arrangements and contents of various museums. On Friday another three and a half hours were given to papers and discussions, followed by visits to museums afterwards. And yet the writer of the note speaks of five hours devoted to business, and talks of papers being burked! All the papers in the programme were submitted to the meeting, except one by a member who, although invited two months beforehand to give notice of his paper, sent no intimation of it till the programme was printed, and then appeared with some notes which he wished to descant upon, and was allowed to do so as long as time permitted. Exception may possibly be taken to a day's excursion finding a place in the programme of the meeting; but as curators have few facilities of exchanging ideas one with the other, generally to their mutual advantage, the value of such an arrangement will, we think, be quite evident to most people, and we believe that "shop" was the predominant subject of conversation, even on that day. The Local Committee in Dublin, in fact, showed the truest spirit of hospitality in affording to the Association every opportunity of prosecuting its work in the direction of acquiring knowledge of the excellent museum work that is carried on in that city, and if the writer of the note thinks that no good can accrue from such meetings, except what is derived from the reading and discussion of papers, he will find few people to concur with him in such a narrow and baneful view. It may interest him to know that the volume of proceedings of the Dublin meeting will be the largest that has ever been published by the Association, and the Local Committee are finding the funds for providing illustrations to some of the papers that were read.

H. M. Platnauer,
E. Howarth,
Secretaries Museums' Association.

Public Museum, Sheffield.

[We have much pleasure in printing this letter from the energetic secretaries of the Museums' Association. This body does, as we have often pointed out, such very valuable work, that we should greatly regret to injure it by any misstatements. We do not, however, gather that any of our statements are seriously traversed. We stated, correctly, that the time allotted to papers was five hours; we are now informed that seven hours were actually conceded, but this apparently includes the time spent on the formal business of the Association, which we did not reckon. We stated that "considerable complaint was heard at the way in which both papers and discussion were burked": we are now informed that all the papers in the programme were submitted to the meeting; this is quite true, but how many were cut short? how often was really useful discussion checked? and were the complaints we heard from numerous people present throughout the meeting entirely imaginary? We stated that "the insertion of a day's excursion between the two days of meeting proved vexatious to those who could ill spare their time"; we are informed that those who had time to spare talked "shop"; but no attempt is made to contest our statement. We fully agree that inspection of museums is and was a valuable part of the week's work; but waterfalls are not museums. Our only object was to utter a timely warning, so that the large amount of active and useful work hitherto accomplished by this Association at each of its annual gatherings might not be diminished by any misguided generosity.—Ed.]

Dr. J. M. Clarke writes to us with reference to the review of Hall and Clarke's "Handbook of the Brachiopoda," which appeared in our August number (p. 140), saying that the edition which fell into the hands of our reviewer "is purely incidental and accessory to the regular one. The work is published as pages 134–300 of the Eleventh Annual Report of the State Geologist, for the year 1891 (dated 1892, published 1894), and contains 22 lithographic plates." These plates were not seen by our reviewer.

We have received the following note from Professor F. A. Lucas, of Washington:—I was glad to see a note on Mr. Lydekker's new cetacean in Natural Science (August, p. 85). I have not the least doubt, from my acquaintance with whales, that what Mr. Lydekker saw was a humpback (*Megaptera*) lying at the surface and thrashing his fins about, which is a very characteristic trick of the genus.

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, 5 John Street, Bedford Row, London, W.C.

All communications for the Publishers *to be addressed to* Macmillan and Co., 29 *Bedford Street, Strand, London, W.C.*

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NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 33. Vol. V. NOVEMBER, 1894.

NOTES AND COMMENTS.

Change of Address.

WE beg to draw the attention of our readers to the notice that appears under the above heading on the last page of the present number. It is hoped that the new arrangement, which will come in force with the new year, will have the effect of facilitating our own business and our dealings with the public by concentrating the Editorial, Printing, and Publishing Offices at one address, instead of distributing them among three as has hitherto been the case. It was well at first to place ourselves in the kindly hands of so well-known a firm as Messrs. Macmillan, and we should like to take this opportunity of thanking them for the assistance and courtesy that they have extended to us. But now that we are growing up and begin to feel our legs, we are bold enough to think that we can walk alone. Whether we can succeed or not must of course depend not on ourselves but on the number of hands stretched out to help us. Since the Review will still be conducted by the same editorial staff and on the same lines as heretofore, we appeal confidently to our readers, subscribers, and contributors not to desert us; indeed, we venture to hope that they will now aid us all the more both in purse and person. If only their sympathy be assured, we shall step from the cradle with a light heart.

Marriage Customs of the Octopus.

In a recent number of the *Archives de Zoologie Expérimentale*, Mr. Emile G. Racovitza relates in a most interesting manner the observations that he has made on the habits of *Octopus vulgaris*. Some previous writers, especially Dr. Kollmann, have imagined that the love affairs of the Octopods are carried on in a barbarous, not to say a brutal manner, and that the resistance of the female to the male

occasions a savage combat between the two. Mr. Racovitza, however, assures us that there is nothing more than a courteous flirtation, and that "the male behaves with a certain delicacy towards his companion." Placed at a considerable distance from the female, the male stretches out the third arm on the right, which is known as the hectocotylised arm, and with its extremity he caresses her, eventually passing it into the chamber formed by the mantle. The female contracts spasmodically, probably because the arm touches the gill, but does not attempt to move. The animals remain in this passive condition for an hour or more, but at about half-time the male shifts the end of the hectocotylised arm from the opening of one oviduct to that of the other: this gives rise to fresh contractions on the part of the female, and at the same time the male has been seen to eject a thick cloud of ink. Should the female prove at all refractory, the male stiffens one of the arms of the second pair, and raises it with a warning motion, as a schoolmaster might raise his cane. With the same gesture the Octopus, turning black with anger, threatens any intruders, and in both cases the menace is successful. Finally the male withdraws his arm from the mantle-chamber, caresses the female with it for a few moments, and then places it in the same position as his other arms.

As regards the actual mechanism by which the spermatozoa are transferred from the body of the male to the ova, Mr. Racovitza has also something new to add. It is well known that the spermatozoa of Cephalopoda are ejected in special tubes or cases called spermatophores; and it is generally stated that the male introduces the end of his arm into the mantle-chamber to take a packet of spermatophores, which he then places in the mantle-cavity of the female. This statement, however, is not based on direct observation: "It has sprung," says Mr. Racovitza, "from the simple imagination of a single author, and has been repeated by everybody without having been brought to the test of either facts or logic, just as is so often the case, especially in the natural sciences." The true facts, so far as *Octopus vulgaris* is concerned, appear to be as follow. The spermatozoa are contained in the round head of a tubular spermatophore. The spermatophore is expelled from the opening of the penis, which stretches out, through the mantle-chamber, into the funnel, which then conducts it to the base of the hectocotylised arm. Down this arm there runs a channel or gutter, lined by smooth epithelium, which is thrown into numerous minute folds. Wave-like contractions traversing the walls of the gutter force the spermatophore along this channel to the distal end of the arm, which, as already stated, is placed in the mantle-chamber of the female. The flattened end of the arm then applies the head of the spermatophore to the opening of the oviduct, in which position it comes in contact with the sea-water and bursts, so that the spermatozoa are set free into the canal of the oviduct. The walls of this canal are provided with numerous

and complicated folds, in which the spermatozoa lodge until the downward passage of the eggs allows fertilisation to take place.

Mr. Racovitza has also studied the habits of *Sepiola rondeletti* and *Rossia macrosoma* at the same interesting period of their lives, but the complete account is not yet published. (See *Comptes rendus de l'Acad. d. sci.*, cxviii., p. 72.)

The Habits of a Crab.

In another part of the paper above referred to, Mr. Racovitza has a note on the habits of *Pilumnus hirtellus.* This little crab lives in holes in the rocks at the bottom of the sea off Cape Abeille near Banyuls. In other holes of the same rocks live some small bivalves of the genus *Tellina.* So long as the molluscs remain quiet, with their shells closed, the crab takes no notice of them. But if one of them moves, then its shell scrapes the rock, and the vibration is communicated to the crab in his hole. After listening for a few seconds the crab sallies forth, and proceeds in the direction of the *Tellina*; he then feels for the hole with his claws and, inserting one of them, draws out the mollusc, which he carries back to his own den, cracks open, and eats.

These observations and a few simple experiments show that by means of its otocyst, which is an organ for feeling vibrations rather than for hearing, the crab perceives not only the presence but the whereabouts of the mollusc. This perception is retained in its memory after the vibration has ceased. The actual spot is found not by sight but by touch. The actions of the crab are purely instinctive and show no signs of intelligence; for any object that is used to scratch the surface of the rock will attract and be seized by the crab, even many times in succession, just as much as if it were a toothsome morsel.

"The Toilers of the Sea."

We have devoted some space to these observations of the Roumanian naturalist, not so much because of their intrinsic importance, though that is not slight, as because they exemplify a side of zoological study that deserves more attention and more encouragement than, in this country at all events, it has yet received. It is hardly necessary to point out that there are many structures the true meaning of which will never be apprehended by one who relies on anatomy alone, if indeed they do not escape his observation altogether. Everyone admits, in theory, the necessity for close and long-continued study of animals living in their native haunts; and yet many a zoologist obtains degrees and honours without having seen a single living individual of the animals that he has written about. It is not to be supposed that this state of things exists by the choice of the zoologists. So far as the ornithologists, lepidopterists, and such workers are concerned, the objection cannot, as a rule, be raised. It is the student of marine animals, especially of those that live in

deeper waters, who so often owes all his knowledge to the scalpel and the microtome.

It is partly with the object of remedying this state of things that marine laboratories have been built in many parts of the world, and many of these offer admirable facilities for the prosecution of a veritable biology or study of life in living organisms. But the mere existence of these laboratories is not the only thing required, and this is easily shown by the contrast between our own and other countries. To make the comparison simple, we need only draw attention to two marine laboratories, both on the shores of the English Channel and on exactly the same meridian, but one in England and one in France —namely, the establishments at Plymouth and Roscoff. At Plymouth the coast is more favourable, the fauna is richer, while the building is finer and more richly equipped than is the case at Roscoff. From the Plymouth laboratory there proceeds every year a large quantity of work of much practical value to the English fisheries; so far as these observations are concerned, we compare very favourably with our neighbours. But when we consider the amount of purely biological investigation that is carried on at these laboratories, and the numbers of zoologists that come to them, a calm examination of the facts will show a very large balance on the side of the French. The Roscoff laboratory is thronged every summer, not merely with students, but with investigators from all parts of the world, including England, while the same may be said of the corresponding laboratory at Banyuls in the south of France.

There may be various reasons for this state of things; but there are two at least which are perfectly obvious. The first is the simple question of cost. To work for a month at Plymouth the student must pay £5 for his table, he will be charged extra for all above a small allowance for material and reagents, he will have to take lodgings in the town, and, when he leaves, he will doubtless make some suitable present to the attendants. At Roscoff during the same period, the student is charged nothing for the use of the table, he is permitted as much of the various reagents, even of absolute alcohol, as any reasonable man can want, he can have a bedroom at the laboratory, and, when he leaves, he will only be expected to leave behind him ten francs for the attendant and fifteen francs for the servant that looks after his bedroom.

The second reason is that the Roscoff laboratory is not an independent establishment, but is intimately connected with the Sorbonne and managed by one of its eminent Professors, H. de Lacaze Duthiers. Consequently the pupils of the Sorbonne go there to work before they have passed their Licentiate examination, and naturally continue to work either there or at Banyuls when preparing for their doctorate. With this nucleus it is not so difficult for the authorities to open their doors in the above-described hospitable fashion to all *bonâ fide* investigators, whose presence is as great a

benefit to the students as it is to themselves, and who naturally make many observations of practical as well as scientific importance on the marine fauna of France.

It is not the first time that we have called attention to the desirability of closer co-operation between the educational and scientific establishments of our own country; and this seems to us a field in which a nearer union is not only perfectly feasible, but would prove to the great advantage of all concerned. It has long seemed to us an astonishing thing that our principal Universities do not insist on their biological students spending at least one summer term at a marine laboratory. There can hardly be any other country where the facilities are so great and where so little advantage is taken of them. The enlightened action of the French Government we have already admired; Professor Haeckel takes his students every year to Heligoland; the Americans naturally are up-to-date with their numerous stations and summer excursions for students; even in Japan, as readers of Mr. Bather's account in our February number will remember, the biological students are required to pass at least one term at the laboratory of Misaki.

Perhaps if similar arrangements were made in England it would be possible, and even as things now are we do not see that it would be impossible, for the Marine Biological Association to offer a little more encouragement to students. The man of science is rarely rich. To do scientific work at all often means the loss of much money to him. Surely such an Association should subsidise rather than tax the struggling investigator. Inspection of the balance-sheets of the Association shows that the amount of money which it yearly derives from the fees or purchases of visiting zoologists forms a very small proportion of its income. The slight pecuniary loss that might be entailed by such reforms as we venture to suggest would be more than compensated by the increased number of visitors and the consequent increase in both value and amount of the scientific and practical observations made at the Plymouth laboratory. By all means let more money flow into the coffers of the Association; but—we put the question to the public, to the universities, and to the Government of this country—is it logical to ask people to pay for the privilege of doing useful work for which they themselves receive no pay?

In the China Sea.

In the centre of the China Sea is a great bank about 80 miles long and 30 miles broad, which is known as the Macclesfield Bank. It rises rapidly out of deep water, and lies at an average depth of 40 fathoms. As it was frequently crossed by vessels, the Hydrographic Department of the Admiralty undertook a more detailed survey, a piece of work which has been accomplished by H.M.SS. "Rambler," "Penguin," and "Egeria," in the springs of 1888, 1892,

and 1893 respectively. In the course of these surveys special attention was paid to the biological conditions of the bank, in order to throw light on the general question of reef- and atoll-formation. The scientific portion of the work was fortunately superintended by Mr. P. W. Bassett-Smith, surgeon, R.N., and the three reports that he sent home have now been published together by H.M. Stationery Office, with a preface by the Hydrographer.

"The general result of the whole examination," says Captain Wharton, "may be stated to be that on the whole of the 200 miles forming the periphery of the bank there exists a rim of coral in luxurious growth, and at a remarkably even depth below the surface of from 9 to 14 fathoms, this rim being broken here and there by passages of greater depth, but less than the general depths of from 40 to 48 fathoms which prevail over the whole central body of the bank. On one spot only on the rim was a depth of as little as 6½ fathoms found, and on a patch in the centre of the lagoon, a small spot of 5 fathoms, the shoalest water on the whole bank was found. This evenness of depth is the most striking feature of the chart, and when the great distances are considered, this appears to be a strong argument against any movement of the bottom since the atoll form was assumed. It is at any rate quite evident that from the present time onwards no movement is necessary in order to form in the future a perfect atoll, the simple growth of the coral on the rim sufficing; and that we may have here an instance of a suitable original foundation for an atoll so formed, as pointed out by Mr. Darwin."

Confirmation of the above opinion will also be found in the reports of Mr. Bassett-Smith himself. Thus, he says, "there appears to me no evidence of either subsidence or elevation of this reef other than that brought about by the building up on a submarine mound of both living and dead forms of calcareous secreting organisms from the animal and vegetable kingdom." And again: "Altogether forty-one genera of corals were represented, excluding the alcyonarian and hydroid corals, viz., *Tubipora*, *Millepora*, *Stylaster*, and *Distichopora*, giving a total of twenty-nine genera in depths between 25—35 fathoms, and twenty-seven in over 35 fathoms; but out of these several cannot fairly be called reef-building, as *Leptoseris*, *Psammocora*, *Anacropora*, and *Balanophyllia*, with *Cycloseris* and other simple corals. With these facts before us there can be no reason to doubt that the actual increase of solid calcareous rock from these marine organisms requires a much less narrow limit of depth than is usually assigned to them, and helps very materially to explain the great depths of these lagoons, which has been up to the present time one of the most vexed questions with regard to the formation of these atolls, and one of the strongest points in support of Darwin's theory. Together with these growing corals in 30—50 fathoms (as pointed out before) exists an enormous amount of rock formed by calcareous algæ *in situ*, and probably the coating on the surface of the living

tissue protects considerably the rock beneath from the solvent action of the carbon dioxide, which likewise is split up and used by the plants themselves. On the lagoon flat very little algæ is present."

We particularly wish to direct the attention of zoologists to these reports, for they contain a large number of interesting observations, which, from the form in which they are published, are extremely likely to escape notice. The paper appears, in fact, to have been issued in March of this year, though it has only just fallen into our hands. It is illustrated by a chart, two outline sections, and four woodcuts.

Echinoderms of the Macclesfield Bank.

As another result of these important surveys, and especially in consequence of the labours of Messrs. Bassett-Smith and J. J. Walker, a very large number of zoological specimens have found their way to the British Museum. The Echinoderms, which present several features of interest to the specialist, have just been described in a suggestive paper by Professor F. J. Bell. Very noticeable in this collection, as also in the collection of Crustacea, is the large number of young specimens. "In fact," writes Mr. Bell, "I have had forced on me the conviction that Macclesfield Bank is a nursery; with a rim submerged 9 fathoms beneath the surface, any pelagic larvæ that will can enter its boundary; being 76 miles long and 36 miles broad, it affords some opportunity for the larvæ to settle." The importance of a large number of young specimens is very great, for not only does it enable us to study the changes that occur during growth, and thus to determine more satisfactorily the relationships existing between species, but it often leads to a happy reduction in the number of species by the discovery that independent names have been applied to what after all are only immature individuals. Mr. Bell therefore wisely suggests that as full collections as possible should always be made in areas resembling the inside of this reef.

Although Holothurians are common on the shallower reefs not far distant, yet they are almost totally absent from the sand-flat of the lagoon; only two young specimens were dredged, here referred to *Colochirus tuberculosus*.

The Crinoids are represented by twenty-two species, of which by far the most remarkable are the new species *Antedon bassett-smithi* and a fragmentary *Actinometra*. The late Dr. P. H. Carpenter, who devoted many years to a study of the free-swimming Crinoids, elaborated a scheme of classification based on the mode of union of the arm-ossicles, which scheme has hitherto been found to work very well. These two specimens, however, run directly counter to nearly all Carpenter's generalisations on the subject. Mr. Bell, who has himself an excellent right to be heard on these particular points, cannot believe that the specimens are merely abnormalities,

and it is clear that his faith in the method of classification according to fixed joints or "syzygies" has been rudely shaken.

The Starfish, numbering twenty-one species, present nothing so remarkable. The drawing of the plates of a young *Culcita* is, however, of considerable interest; but the questions of homology which it raises could hardly be settled from the evidence of one ray alone, which is all that Mr. Bell has chosen to give.

Among the eighteen Ophiuroids, the most striking is one with branched arms, like an *Astrophyton*, but possessing plates on its disc which closely resemble those of the ordinary Ophiurids. This forms the type of a new genus, known by the ingeniously derived name, *Ophiocrene*. But for the meaning of this name, and for the many questions raised by this specimen, we must refer our readers to the original paper in the *Proceedings* of the Zoological Society for 1894 (pp. 392–413, and pls. xxiii.–xxvii.).

Fresh Forms of Foraminifera.

Quite a crop of papers on these organisms has recently appeared. Mr. F. Chapman continues his studies on the Gault forms in the *Journal of the R. Microscopical Society*, in dealing with the Vaginulinæ. No less than thirteen varieties are described and figured, most of which have been previously recorded by Reuss from Germany and Berthelin from France, but one (*V. priceana*) is new. Mr. E. Halkyard has a general and useful paper, on "Plans of Growth" and form in the Foraminifera (*Trans. Manchester Microsc. Soc.*, 1893), in which he collects together much scattered information, his object being to show the diversity of shape and how one form runs into another among these lowly-organised beings. The paper is illustrated by two plates which considerably assist the text. Walter Howchin continues his work on the Foraminifera of Australia, and contributes two papers. The first is "On the Occurrence of Foraminifera in the Permo-Carboniferous Rocks of Tasmania" (*Austral. Assoc.*, Sept., 1893, Adelaide), with two plates of rock-sections. He has not yet succeeded in isolating specimens, and consequently our knowledge must at present be limited to genera. His second paper is entitled "A Census of the Fossil Foraminifera of Australia," and gives a list of all forms recorded up to 1893. Two hundred and seventy-three species are listed, and these are again tabulated under their stratigraphical position. The paper, which was published in the periodical referred to above, will be of considerable value, but the publication of MS. names is to be deplored, as it can only be injurious to science.

Dr. Franzenau, of Budapest, writes on "Die Foraminiferen des ober-Mediterranen Tegels von Zsupanek" (*Természetrajzi Füzetek*, xvii., 1894), from which he records fifty-four species, three of which seem to be new, though they are but briefly described, and not figured. Mr. Chas. Schlumberger continues his excellent work on the Miliolinæ

by describing and figuring (*Bull. Soc. géol. France*, xxii., 1894, p. 293) a new *Lacazina* (*L. wichmanni*), which he has recovered from fossil deposits on the north-east of New Guinea. In a second paper, "Note sur les Foraminifères des Mers arctiques Russes" (*Bull. Soc. zool. France*, vii., 1894, p. 252), he describes and figures *Triloculina pyriformis*, *Quinqueloculina parvula*, *Sigmoilina herzensteini*, *S. macarovi*, *Reophax flexibilis*, *Lagena serrata*, accompanying his descriptions with those exquisite sections students know so well. The specimens described were obtained from Kola Bay (Kildin Island), and the Sea of Okhotch, and were dredged by Admiral Macarov during one of the recent Government expeditions to the Mourmane peninsula.

Professor Andreae has completed the work begun in 1870 by Von Schlicht and Von Reuss on the Foraminifera of the Septarien-Thon by a paper on these deposits near Frankfort a.M.; he adds notes on the vertical distribution of the Foraminifera therein contained (*Ber. Senck. nat. Ges.*, 1894). Federico Sacco has paid attention to the Miocene Tinoporinæ of Turin (*Bull. Soc. belge Géol.*, vii., 1893), and found it necessary to make three new genera, *Miogypsina*, *Baculogypsina*, and *Taurogypsina*. The first two are founded on previously known forms, but there seems some doubt in the author's mind whether *Taurogypsina* is a Foraminifer or not.

The Life-History of Some Foraminifera.

So little biological work has been attempted on this group that we hail with pleasure Mr. J. J. Lister's contribution to the above subject in the *Proceedings* of the Royal Society (vol. lvi., 1894, pp. 155–160). Mr. Lister has studied the phenomenon of dimorphism, which shows that individuals of a species fall into two groups. In one the central chamber is of considerable size (megasphere), while in the other it is small (microsphere). The two forms differ also, in the Miliolidæ for instance, in the plan on which the chambers are arranged, in the size attained by the adult shell, and in the frequency of occurrence.

The forms known as *Polystomella crispa* and *Orbitolites complanata* were examined, and permit of the following summary and conclusions:—

1. The species are in a great number of cases dimorphic.
2. The two forms differ from each other:
   (*a*) In the size of the central chambers.
   (*b*) In the shape and mode of growth of the chambers succeeding the central sphere.
   (*c*) In the character of their nuclei (size, etc.).
3. The megalospheric form of a species is much more numerous than the microspheric.
4. The megalospheric form has been seen to arise in at least seven genera as a young individual already invested by a shell, produced in

the terminal or peripheral chambers of the parent. While in some cases (*Orbitolites*) the parent of such a megalospheric young was microspheric, in others (*Peneroplis*, *Orbitolites*) it was megalospheric.

5. Foraminifera, in certain conditions, give rise to active swarm-spores.

The two forms may be safely concluded, says Mr. Lister, to be distinct from their origin, and his observations leave it impossible to regard the difference as sexual. Brandt has recently described a similar nuclear history in *Thalassicola*, one of the Radiolarians, and in this group the individuals of a species fall into two sets, those producing isospores and those producing anisospores, which are regarded as an asexual generation alternating with a sexual; and Brandt's observations present resemblances to the life-history of *Polystomella* as observed by Lister.

So much for the microzoa of the sea. Let us now glance at the macrozoa of the land.

The Extermination of Big Game.

In an interesting article in the October number of the *Fortnightly Review*, Mr. H. A. Bryden, whose book entitled "Gun and Camera in South Africa" is known to all students of African natural history, laments the terribly rapid rate at which the game is being exterminated. He reminds us of the vast herds that roamed over Cape Colony in the days of the earliest Dutch settlers, when lions swarmed so at Cape Town that the Governor thought one night that they "would take the fort by storm"; when antelopes ravaged the crops so that some districts could hardly be cultivated. That a little further inland this condition of affairs lasted till the present century, Mr. Bryden shows by extracts from the journals of the great hunters. Thus Dr. Andrew Smith saw not far from his waggons one hundred and fifty rhinoceroses in one day, and one hundred giraffes almost at the same time. At the present time many of the finest animals are extinct either altogether or locally, and far up the country, frequented only a few decades ago by herds of noble game, Englishmen are now reduced to hunting with foxhounds the jackal and that tiny antelope the "duiker."

Mr. Bryden gives an account of the various agencies that have led to this change and of the war waged against the game by Dutch farmers, native hunters, and the great English sportsmen, Smith, Harris, Oswell, Gordon Cumming, Baldwin, and Selous. He admits that the sportsmen, though often wasteful, have been a comparatively insignificant cause of the extermination, and pays a warm tribute to Selous for his merciful methods of hunting and his economy of life. Mr. Bryden, is, however, quite clear that the great game is doomed to annihilation, and that many species will soon share the fate of the quagga, the bontebok, and Burchell's rhinoceros. He

suggests that a great game park should be established in or near Mashonaland, where 100,000 acres should be fenced in as a preserve. The enforcement of game laws in unsettled districts he admits to be impossible, while the regulations of the British South Africa Company show that it thinks those laws unjust. While prospectors, explorers, and natives are continually killing for food, and while cattle disease sweeps at intervals through a country, destroying every animal of certain species within it, the toll levied by sportsmen is a very unimportant factor in extermination. The sportsmen, too, as Mr. Bryden admits, have done such good work as explorers and pioneers, and have left among the natives so high a respect for British pluck, truth, and fairness, that it is very ungrateful to blame them. It is true that they may have helped in the extermination of some species, but it is also true that, except for their efforts, many species now exterminated by natives and settlers for skins or for the pot would never have been known to science.

Current African Exploration.

After considering the past destruction of the game of the Cape, it is natural to turn to the work of modern hunters in those parts of Africa where great herds of game still survive. With the return of winter the usual shooting excursions into Africa have started, or are preparing to start. Somali-land is, as usual, the favourite district. Sir Henry Tichborne and Mr. Seton-Karr have left for Berbera, whence they march at once across the Haud to Milmil; thence they hope to make excursions southward and westward, and finally shoot slowly back to the coast. Prince Boris, well-known from his daring expedition to Mount Meru, which he was one of the first Europeans to reach, is preparing to attempt a march from Berbera across the Galla country to Lake Rudolph: his trip in the winter of 1892–3 with the Duc d'Orleans across Somali-land into the northern borders of the Galla country, will have served as a most useful training. It is greatly to be hoped that he will reach his goal, for knowledge of this country is the greatest desideratum in African geography. The last effort in this direction has not been very successful, if the native rumours are to be trusted; for, according to one of these, the expedition of Dr. Donaldson Smith, which left Bulhar in July, has been refused permission to cross the Galla frontier. Apparently Dr. Smith either did not feel strong enough to use force, or did not think himself justified in doing so. We must hope that the rumour is false, for it will be very disappointing, both to Dr. Smith, his companion Mr. Gillett, and those at home interested in African exploration, if his generous expenditure of time and money fails to reap the reward it deserves.

Mr. Greenfield, whose arm was so badly mangled by a wounded

lion in Somali-land that his life was for some time in danger, is, we are glad to state, home again, and well on the way to recovery. The latest news from Mr. Scott-Elliott is still that published in our last number. An account of his march from Uganda to Ruwenzori has been published in the *Geographical Journal*, and a letter giving some notes as to the flora of Ruwenzori has been printed in *Nature*. As more is expected from this expedition than from any other in the field, great anxiety will be felt for Mr. Scott-Elliott's safety during his return march to the coast.

Wasted Wealth in India.

The September number of the *Kew Bulletin* contains some interesting extracts from a memorandum on the Resources of British India recently prepared for the Indian Government by Dr. G. Watt. The British India of the memorandum covers an area of 699 million acres, with a population of about 222½ millions. The agricultural products are considered under six heads—Food-crops, such as wheat, rice, pulses, sugar, spices, etc.; Oil-seed; Fibres; Dyeing and tanning materials; Drugs and narcotics, etc., including tea and coffee; and Miscellaneous, such as cutch, lac, indiarubber, and other wild products. *A propos* of the last-named group, Dr. Watt lays considerable stress on the neglect of the wild or semi-wild animals and plants as a source of wealth. India has borrowed far more than she has given, and the improvements of the future, it is suggested, should lie as much as possible in the path of natural selection and evolution of indigenous materials and systems. These can be counted by the hundreds, and India need not look to foreign countries for new crops while she has a long list of unexploited products which are running to waste in every lane and jungle. An extension of the effort to bring these hitherto unknown products (unknown, that is, to European commerce) into a position of definite recognition is more worthy of serious consideration than the attempt to acclimatise the plants of other countries. The people might be encouraged to grow as hedges round their fields, not useless plants with the sole recommendation of rapidity of growth or efficient protection, but bushes valuable as fuel, or as sources of dyes, tans, fibres, and the like. Already, in the Bombay Presidency, miles of roadsides have been planted with the useful and ornamental *Cassia auriculata*; but in Western and Central India thousands of square miles are overrun by scrubby bushes of *Anona squamosa*, serving at present no useful purpose, though the fibre from the bark might be valuable. *Bauhinia Vahlii*, of which the fibre can be bleached and dyed by the same processes as wool, is a common climber in the jungles of the lower hills, and might, at little cost, be cultivated over rocky country at present next to useless.

Fishing for Coal at Calais.

Nearer home there is another source of wealth that has long lain unworked because unknown, namely the beds of coal that are now believed to underlie the south-east of England. We have lately received important information bearing on this subject.

Stimulated by the discovery of coal at Dover, our neighbours across the Channel bethought themselves, some time since, to make a *forage* near Calais. The proximity of the small coal-field in the Boulonnais, and the circumstance that the spot selected for the experiment was practically on the line of strike of known productive Coal-measures, were also factors greatly in favour of the undertaking. The results, although interesting from a scientific standpoint, are not altogether encouraging in the commercial sense. For we learn that at a depth of 325 mètres the boring tools have entered rocks of Devonian age, as proved by the occurrence of fossils.

Mr. Gustave Dollfus, of the Geological Survey of France, who has carefully watched the progress of the work since its commencement, is of opinion, we are informed, that these Devonian rocks are inverted, notwithstanding the fact that they are horizontal. Judging from the disposition of the upper Palæozoic rocks along the axis of the Ardennes from the neighbourhood of Liège, through Namur and Mons to their underground prolongation into the Pas de Calais, it would seem that Mr. Dollfus possesses sound arguments in favour of his hypothesis, though he has not yet put them forward. In the last-mentioned locality, as at Liège, the Coal-measures are found beneath the Devonian. The Palæozoic beds concerned are much faulted, folded, and at places inverted. These phenomena appear to have been caused by the influence of a force which acted in a direction from south to north, while the chief disturbances resulted in the formation of oblique faults. The most important of these is the great Eifelian fault which bounds the main coal-basin on the south. In many places the Lower Devonian of the basin of Dinant, abutting against the ridge of Condroz, has been thrust over the coal beds of the Namur basin, and this together with the inversion of the Middle and Upper Devonian, Carboniferous Limestone, and part of the Coal-measures themselves has much complicated the stratigraphy.

In these conditions it would be perfectly possible to find the Coal-measures at Calais under either inverted or normally-disposed Devonian, though had the boring been situated rather more to the south, the probabilities would, in our opinion, have been greater. When other borings than the one near Dover reach Palæozoic rocks in Kent, we shall not be surprised, therefore, to learn that these latter are Devonian, and the discovery of beds of that age in the London area does not altogether do away with the possibility of the occurrence of Coal-measures in the neighbourhood of the metropolis. It has long been recognised by geologists that the old Palæozoic floor, or "ridge," under London has much in common with the axis of the Ardennes.

"Mente et Malleo."

It has often been laid down that the importance of any international congress of scientific workers depends, not on the novelty of the facts or ideas brought forward by individual members—for such are really more accessible if published in one of the recognised periodicals—but on the methods inaugurated for facilitating international communication and co-operation, and for assisting the students and investigators of the science in question, wherever on this earth they may be working. Rules of nomenclature, the organisation of research, the publication of bibliographies: such are the subjects that a great world-congress can worthily and usefully occupy itself with. But since members are but men, and since a congress must assemble in some definite place, it often happens that friendly intercourse, widening of sympathies, increase of knowledge, and the dispelling of ill-founded prejudice are among the most valuable results of such a gathering. But these latter results are subsidiary to the main functions of a congress, and they are by no means inevitable; the worth of a congress that summons its members from the ends of the earth has therefore to be estimated not so much by these accessories as by the solid work that is accomplished for the advance of science and to the benefit of those members who, though they pay their subscriptions, may be unable to attend in person. It is by this standard that we must judge the sixth triennial International Congress of Geologists that was held at Zürich a few weeks ago.

One of the most important tasks that this Congress has undertaken is the preparation of a geological map of Europe on the scale of 1 : 1,500,000, and contained in 49 sheets. The objects of this map are to summarise and correlate the facts accumulated by the various surveys, societies, and individuals, and to represent them on a system that shall, if possible, serve as a pattern for the geologists of the world. It was therefore with gratitude that we heard that six sheets of this map were at last ready for publication, and that ten more would be issued within a year's time. As the subscription price of the entire map is only £4, which may be paid in instalments, the first part being 10s., it is to be hoped that many will send their orders to Dietrich Reimer of Berlin, which they should do before the end of December.

Another useful bit of work that will soon be completed and distributed to members both of this and of the previous Congress, is the "Catalogue of Geological Bibliographies." We understand that this compilation has far exceeded the bounds originally determined for it, and it remains to be seen how far it has been improved, or damaged, by this unusual zeal.

The committees started by the present Congress are two. The nomenclature of rocks has been getting into as sad a state as the nomenclature of animals. Consequently, at the suggestion of Michel-Levy, who himself spoke admirably on the subject, a committee has

been appointed to revise and unify the nomenclature. The English members of this committee are Sir A. Geikie, Professor Judd, and Mr. Teall; other countries are likewise represented by their most eminent petrologists. The changes that occur in glaciers are to be the subject of investigation by another committee, which may possibly also organise the study of erratic blocks. Professor F. A. Forel and Dr. L. du Pasquier will be the secretaries of this committee, the expense of which will be borne by Prince Roland Bonaparte. The English representative is Captain Marshall Hall.

Among the papers read at the Congress there were many worthy of the occasion, such as that by Michel-Levy already referred to, Von Zittel's address on Phylogeny, Ontogeny, and Classification, and Suess's address on the Conformation of the Crust of the Globe by Horizontal Forces. We must, however, express our regret that it was thought necessary to split the Congress into four specialised sections, a course that annuls many of the benefits otherwise derivable from such an assembly, and that would not be dreamed of if only some discretion were exercised as to the subjects of papers. While it is very right and proper that such a Congress should discuss the investigation of overthrusts, the classification of Tertiary terranes, and, perhaps, points of unusual interest in the geology of the country where it is meeting, still we fail to see the appropriateness of such comparatively trivial subjects as the Fossil Camel in Roumania, the Geology of Patagonia or Palestine, Nepheline-Syenite of Sweden, Gabbros of Lower Italy, and "grorudites and tingnaites" (the quotation is from *Nature* of Sept. 20, where a more detailed report will be found).

We had intended to say something about the social and administrative details of the Congress, but we are spared a difficult task owing to the fact that what seems to be a singularly outspoken private letter from Mr. Persifor Frazer has somehow got published in the October number of the *American Geologist*. Had the account been intended for the public eye, no doubt it would have been neither so truthful nor so entertaining. It is, however, refreshing, as well as alluring, to see sometimes the naked truth, and in the present instance those who have the interests of the Geological Congress at heart will not regret the absence of the conventional fig-leaf.

The Climbing Rabbit.

We are indebted to the Rev. George Henslow for an extract from an article in the *Hobart Mercury* dealing with the subject of the adaptation of animals to their surroundings, of which we gave a new instance last month (p. 249). The article records some curious facts concerning the development of a new sort of nail in the rabbits of Australia in consequence of the animals' endeavours to climb over the wire netting used to impede their progress in travelling. The farmers have discovered that the rabbits can

burrow under the netting unless it is buried some six or eight inches under the soil. Moreover, they can climb, or evidently intend to do so, after a little training, and to this end they are developing a nail which will enable them to hold on while progress is made upward. This nail development was noticed before this in Queensland, when the bark just out of reach was desirable of attainment, but to effect hand-over-hand nautical climbing shows the rabbit in the act of elevating himself in the scale.

The Gay Science of Newspaper Criticism.

While the provincial Press openly devotes part of its space to an amusement column, some of the London daily papers afford the same relaxation to their readers in the shape of Science Notes, from which we have on former occasions culled choice specimens. The *Daily Chronicle* in particular has distinguished itself by providing a weekly column of mixed information and criticism. Its writer is especially happy in his observations on the scientific Press, and his remarks on a contemporary of our own must have provided a source of innocent merriment to a large body of readers.

Some time ago we suspected the author of these notes of a certain admirable power of divination, which served him in place of reading the memoirs he criticised. There were displayed a striking unfamiliarity with the contents of the memoirs in question, and a disposition to be hard on the "poor English" in which they were written. This last is plainly a matter on which the writer of the notes is an authority, from practice. His observations on the magazine in question for the current month were therefore pleasantly anticipated by the readers of the *Chronicle*, and their reward is great. Among other interesting things, he tells us that in the pages of our contemporary "mountain sickness is intelligently treated by Professor Roy." We are extremely sorry for Professor Roy that he should lose this approbation, but unfortunately the magazine does not contain an article by him on the subject—*although one was advertised.* We read also that "though Mr. Hawkins on continuous-current dynamos and Mr. Moore on the morphological value of the attraction sphere are stiff reading, they will be valued by physicists." It is quite plain that the morphological value of the attraction sphere furnished very stiff reading indeed in a certain quarter, and Mr. Moore, we fear, will have to postpone the delights of appreciation by *physicists.* The *Chronicle's* complaint of the "unnecessary display of technical language" in scientific papers becomes intelligible. Its editor might do worse than present the contributor of "Science Notes" with a dictionary and a treatise on commercial rectitude—or recommend him to a sporting journal, where gifts of insight and prophecy have a value of their own.

I.

On Random Publishing and Rules of Priority.[1]

AMONG invertebrates the species of insects are reckoned by millions. Of crustaceans there are several thousands of species. There are thousands of species also of spiders. To whatever minute subdivision of the animal kingdom zoologists turn their serious attention, the record of species in it presently rises to scores and hundreds. That a large proportion of these are improperly founded, representing only duplicate names, casual variations, differences of age and sex, or errors of the observer, may be readily conceded. But with every allowance of this kind, the irreducible remainder constitute an enormous multitude. For purposes of study and discussion, it is essential that they should all have recognised names and a place in some system of classification. In the rapid advance of knowledge, system is displaced by system in quick succession. The monographic work designed to bring into one view all the known species of a group almost invariably adds many that were previously unknown. Before such a work can be printed and published, the author's fellow students in several parts of the world will have brought out independent contributions to the subject, thus stamping the monograph with the stigma of imperfection from the very moment of its birth.

Before any comprehensive work on zoology can now be produced, the naturalist finds his studies in a manner divorced from nature. There is so much to read that little time is left for observing. He is in a sort of bondage to an antecedent literature which cannot be ignored, and which comprises often a host of sporadic writings. Under existing arrangements, these scattered essays carry equal authority for their definitions and descriptions of genera and species, wherever, however, and in whatever language published. They may be couched in Russian, Polish, or modern Greek, in Arabic or Japanese. There are linguists to whom none of those languages would present any difficulty, and who would say that one language was as good as another and a great deal better; but even a good linguist might be daunted at hearing that some African sage had described the

[1] Read at the British Association, Oxford, 1894.

fauna of his country in his tribal tongue. So sensitive are some of the most eminent Scandinavian naturalists to the inconvenience of employing a language unfamiliar to the majority of scientific readers, that they write their books in English in preference to using their own Dano-Norwegian or Swedish. We may take it as rather a two-edged compliment to ourselves, implying, as it does, that they are more capable of learning to write our language than we are of learning to read theirs. Notwithstanding this, many readers are duly grateful, and would be only too glad to pocket a similar affront if it were kindly offered them by Russian naturalists.

There is, however, a confusion much more embarrassing to the student than the confusion of tongues. He has to face the perplexing fact that not only may the publication of new genera and species lawfully be made in any one of the numerous countries of the world, but that in any one of those countries there is no limit to the media of publication which may lawfully be employed. A very costly book of travels, with or without other faunistic details, may devote a few lines to a new spider, though containing nothing else whatever that concerns an arachnologist. Some small local society, in the course of its peregrinations, discovers a new earthworm. The description is printed in the local *Transactions* rather for the delight and glory of the members than with any expectation or wish that it should become known to the outer world. A new snail is described in a scientific magazine, and at first sight such a report appears to be exactly in its proper place, until inquiry shows that another new snail has recently been described in a rival magazine of equal scientific pretensions. The *Proceedings* of a Marine Biological Station record a new fish, and again one is inclined to say that such *Proceedings* are precisely the proper organ for making known new fishes, and, were there only one such station instead of an indefinite number, that might be true. Again, there are, in our own country, for example, the great London societies devoted to the promotion of Natural History. They are rich and generous. From a new flea to a new rhinoceros, they give the discoverer all the help in their power to introduce his clients to the knowledge of their fellow-creatures.

In short, no one who is capable of endowing the world with a new species need be at a loss for an opportunity of doing so. When, however, the question arises whether the species be really new to science, the varied facilities for publication wear a less amiable aspect. The conscientious zoologist finds himself in the agreeable predicament of having to prove a negative. It is his duty to ascertain that his proposed species has not been previously described. To abbreviate the investigation, he is often tempted to rely on second-hand authorities, the well-known parents of truth and accuracy. But the position should be considered of those who, with rigid honesty, endeavour to consult original memoirs. Suppose, for

instance, that their researches are concerned with some group of crustaceans. In the literature of that class the great American work by Dana is conspicuous. It has the advantge of not being a farrago, a hotch-potch, a Noah's Ark, a confused miscellany of animals of every kind. It is concerned with crustaceans alone. Nevertheless, within that reasonable boundary, it describes and figures so many species of so many different groups, that almost every carcinologist must at some time or other want to have it at his elbow. But, since only one hundred copies of it were issued, it cannot be every man's book, and since the price of it is twenty pounds, it cannot be within the reach of every naturalist's purse. It is needless to enumerate all the other expensive works of more or less similar character. If these are burdensome, though confining themselves to animals of a single class, what must be thought of books which treat promiscuously of all classes of animals, passing airily from a sponge to a parroquet, from a crayfish to a chimpanzee? Still there is this to be said for large and costly productions, that they are generally well known and not easily overlooked. Far otherwise is it with the great cloud of short essays and pamphlets which equally demand attention. Many of these can only be obtained by diligent search in the catalogues of specialist booksellers, or consulted in some library which possesses the magazines, journals, proceedings, or transactions in which they originally appeared. The latter alternative may seem to involve a very small hardship, but those who have tried it very well know that, whether the naturalist leaves his home to go to the books, or the books leave their home to go to the naturalist, there is delay, there is worry, there is expenditure of time or of money, or of both.

In London alone, papers dealing with Crustacea are accepted by the Royal Society, the Linnean, the Zoological, the Entomological, the Microscopical, and perhaps several others. There are societies similar in purpose to these, and as a general rule equally omnivorous, spread in profusion over all the countries of Europe, throughout the United States of America, and elsewhere. When you have finished studying the *Acts* of the Academy of Rovereto, the *Zeitschrift für Zoologie* of Budapest and the Kansas University *Quarterly*, you may pass on to the *Journal* of the Institute of Jamaica, the *Journal* of the Natural History Society of Bombay, the *Journal* of the Asiatic Society of Bengal, the reports of the Wellington Philosophical Society of New Zealand, of the Linnean Society of New South Wales, the *Anales* of the University of Chili and those of the National Museum of Montevideo. Before you have done, you will have qualified for a Fellowship of the Royal Geographical Society, if only you have found out where all the places are whence all the scientific contributions are issued. Were the student capable of reading the different languages in which they are severally published, he would still be seldom able to afford the time and money required to ensure him an

opportunity of perusing them all. The truth is that, in their zeal and devotion, the labourers at natural history do a great deal of work that is not wanted, of work that has been already done, and of work that is premature. It is, perhaps, almost impossible to place any check upon the performances of the incompetent, but were all that is done of vital importance, the want of any order or method in presenting it to the world would still involve a prodigious waste of time.

It is far easier to point out the evils than to suggest practical and feasible remedies. At the risk of being thought rashly presumptuous, I venture to hint at the possibility of a division of labour among the leading scientific societies. Without any restriction upon the pleasant variety of subjects discussed at their respective meetings, they might be willing to enter into an amicable concert in regard to printing and publishing. By way of illustration, one could imagine that under such a system the Entomological Society would only issue papers treating of insects, which is, I believe, in a great measure its existing practice; the Zoological Society would content itself with essays on the rest of the animal world, and the Linnean would deal only with the science of the vegetable kingdom. This particular apportionment of work is not intended for a substantive proposal, but merely for an explanatory suggestion. There would be other societies to consider, other plans to debate, and many inherent difficulties only to be met by compromise. The Microscopical Society, for example, would naturally say that its work essentially lay in all departments, and could not reasonably be confined to one. Though this would be true of its work, it need not perhaps affect its publications. Such a society might take over the invertebrates from the Zoological, transferring to that and to the Linnean those microscopical investigations which concern the vertebrate animals and higher plants. If only the general idea of the division of labour were accepted, it would be derogatory to men of science to suppose them incapable of devising efficient means of reducing the principle to practice. One might hope that, if a beginning were made, either in this country or in any other, the merits of the system would soon be widely appreciated, and concerted action be taken not only among the societies of a single nation, but among those of a continent.

There would still be the unfathomable deluge of miscellaneous zoological writings spread over books of travel and over countless magazines issued by private enterprise. But upon many or all of these the force of good example and of public opinion might be expected gradually to operate. No one need be alarmed that the proposed concentration of subjects would result in too much narrowness of mind, for there will certainly always be a residuum of magazines aiming at a kind of fragmentary omniscience, giving some information on every possible subject, that every possible customer may find some scientific scrap to suit his favourite taste and particular desire of knowledge.

The British Association, many years ago, rendered a great service to science by recommending the adoption of certain rules for zoological nomenclature. It has assuredly an opportunity of rendering a service of like character, but one of even higher importance and wider usefulness, if it will now take the initiative in devising and recommending methods by which research may be simplified in its literary aspect.

It is true that the scientific world is not left entirely in a helpless condition. The *Naturæ Novitates*, published fortnightly by Friedländer in Berlin, are a substantial assistance. They may be compared to Bradshaw's Guides, which give you a map of the country and tell you what lines of railway there are, what trains run upon them, what are the starting-places, and what is the cost of travelling. Works like the "Zoological Record" may be compared to Murray's Handbooks, which tell you both where to go and what there is to be seen when you get there. Such aid deserves the deepest gratitude. Nevertheless, the "Zoological Record" itself requires the student of the Protozoa to buy, not only the account of the works in which he is interested, but also the accounts relating to all other classes of the animal kingdom, just as if Mr. Murray were to insist that the purchaser of his "Handbook to Portugal" should pay also for those referring to Russia, Denmark, Italy, and the rest of the series. The British Association is, of course, privileged to survey the whole domain of science; nor would any of its members wish to see its liberties curtailed. Yet, as an aside, in a stage whisper, and among ourselves, I may venture to deplore the lot of a palæontologist who wishes to possess the original description of three new species of fossil Estheriæ, occupying a couple of pages in our last volume of reports. Bound up with them, he will have the satisfaction of owning 140 pages containing nothing but numerals, 40 of these pages dealing with "The Pellian Equation," a mystery which a zoologist is likely to study with rapt devotion.

In appealing to the Association to consider whether some order and method may not be gradually introduced into the confused concourse of scientific, and especially zoological, publications, I am perhaps proposing a fanciful, a hopeless, an impracticable task. But even if it should prove that nothing definite could be done, I am persuaded that the discussion itself would be advantageous, by calling the attention of scientific writers to the enormity of the evil demanding a remedy, and the expedience of individual self-denial for the general welfare. In the meanwhile, there are two or three subsidiary points on which it would be easy for the Association to express an opinion, and on which, as it seems to me, such an expression of opinion would not only be valuable but timely.

Many naturalists, I believe, now wish that the tenth edition of the "Systema Naturæ" of Linnæus should be accepted as the starting point of valid nomenclature instead of the twelfth edition. Still

more wish that the recommendation in regard to synonyms should now be made a definite rule, to the effect that any name of genus or species which has once become a synonym should thenceforward not be available for any new genus or species. Another point concerns English-speaking people rather than the world at large. Whether our common English pronunciation of Greek or Latin be the best or worst, it would certainly be a great blessing to those who use it if an effective guide to pronouncing could be furnished by a small improvement in printing. It was long ago suggested, and no better suggestion has ever since been made, that long and short vowels in zoological names should be discriminated by the straight line and the upturned crescent placed above them, as in prosody. From the want of these guiding marks scientific conversation becomes often very confusing. Ninety years ago Sydney Smith observed that a young Englishman who made a false quantity in public at the outset of his career seldom got over it. That day has passed. We no longer mind the false quantities; but in discussion we still want to know what the disputants are talking about. There is sometimes no right or wrong in question. As with the name of a well-known genus of trilobites, so far as the derivation is concerned, it may equally well be Calymĕne or Calymēne, but it surely ought to be definitely one or the other. There is a shell called Nautĭlus, a crab called Lithōdes, a shrimp called Hippolȳte, a spider called Schœnobătes. So far as ordinary printing shows, they may just as well be called Nautīlus, Lithŏdes, Hippolў̆te, and Schœnobātes. When a name *can* be mispronounced it almost certainly *will* be; and then there arises a difficulty of understanding one another between those who use the ear-torturing sounds and those who speak correctly. The adoption of the letter "k" instead of "c" in spelling words of Greek origin would be another improvement, which would help to save such a word as Ceraurus (Kera-urus) from being pronounced Sero͡arus.

Lastly, I desire to suggest that there is one particular department in which a scientific convention might be established with comparative ease. According to existing rules, priority is granted to those who introduce new genera and species only on condition that they name them in conformity with the binomial system, and publish some sort of recognisable description. Wherever the authority resides which has enacted these conditions, the same authority must certainly have the right and power to add to them in the interests of science at large. My suggestion then is this, that, for the valid publication of species, each country, or any group of consenting countries, should have one and only one authoritative journal. Let no new species, however minutely described elsewhere, have any claim to priority until the author has entered its name in the appointed journal, together with a concise account of the characters relied on for its discrimination. The editor of such an organ would be bound to publish at stated intervals all species offered him as new,

unless withdrawn by the consent of the contributors. One can easily imagine that his experience would often enable him to point out with convincing effect that a supposed novelty was not really new, that a proposed name was pre-occupied, or that the proposed form of it was something a little too horribly barbarous.

Were the principle once admitted that new genera and species could not be sprung upon the world from every obscure hole and corner, but must be announced at some appointed and easily accessible centre, it is not unreasonable to hope that naturalists of different countries might consent to combine their forces for the common benefit. It might be found convenient, for example, that all new Mollusca should be published in France or in Germany, all new Mammalia in Italy or in Denmark. As in that case the journals, receiving contributions from all parts of the world, would be a patchwork of many languages, it is probable that the original descriptions would require to be accompanied by a rendering either into the vernacular of the publishing country, or into Latin, or into some sort of scientific Volapük. Such details, however important, it would be premature as yet to consider. It may be that scientific workers will think absolute freedom preferable to the restraints imposed by systematic combination. Yet it seems overwhelmingly obvious that to every student of a special branch of natural history it must be of the highest advantage to know at once where to turn for accredited information on the most recent discoveries in the group which he is studying. It may be at once conceded that the knowledge of new species is only a small fraction of zoological science, but it is almost certain that around the record of new species in a particular group the reports of other investigations in that group will tend to cluster. We are so accustomed to the process of harvesting a stack of wheat from one spacious field, that probably a farmer could hardly think with patience of a different system, by which his reapers would be forced to tramp over half a county, obtaining a bundle of grain in one place and a wisp of straw in another, and from here, there, and everywhere collecting single ears of corn, happy if they chanced to find a fair proportion not green and sour and meagre and mildewed. Why should naturalists continue in a condition of affairs which a rustic would consider almost inconceivably absurd? However great may be the intellectual and physical vigour enjoyed by men of science, and however ample their prospect of life at the outset, they soon have to realise that the work which they would fain do needs more time and more strength than they can possibly hope to bestow upon it. Often, like Alpine travellers, they reach what seemed to be the longed-for summit, only to find that the true mountain-top is still far away and far above them. Their limbs are wearied. The intervening valley is dense with mist. The shadows lengthen. The night is near. They can go no further. The exploration which they, perhaps of all men, were best fitted to accomplish is left unfinished, because, not on

the difficult heights, but in the lowlands and at the outset of the journey, their progress was needlessly hampered and their time pitilessly frittered away. Even those who make no claim to exalted genius, who attempt no heroic adventures, but who with a modest and faithful industry endeavour to do the needful drudgery of science, may ask to be delivered from a state of affairs which more or less compels them to waste their own time and reluctantly to waste the time of others. If it is true in commerce that time is money, it is no less true that it is also one of the chief factors of success in science. Therefore I appeal to this Association to make a small effort for a great purpose, by appointing a committee to consider how the time of scientific workers in general may be saved, through the introduction of method and order into the publication of scientific results.

Thomas R. R. Stebbing.

II.

Miocene Man in India.[1]

BY Professor Prestwich's explanation of the mode and relative date of the occurrence of the plateau flint implements in Kent,[2] we clearly understand that Man inhabited the region comprising what is now the South-eastern part of England at a very remote period ; so far back, indeed, in the earth's history as to require of us the recognition of a vast lapse of time, measured by the duration of slow natural processes, forming, elevating, and removing extensive deposits of marine, estuarine, and fluviatile materials. In the geological chronology, as marked out by fossils and strata belonging to certain stages in the recognised succession of periods and their characteristic formations, that of the "Red Crag" of Essex (formed in a shallow sea in "Pliocene" times, just before the "Glacial Period" began), has been indicated by Professor Prestwich's researches as having been contemporaneous with the condition of things existing in what is now Kent when Men of some primæval kind existed on hill-sides far above where Crowborough and other ridges in the Wealden area now rise to a much lower elevation.

In France flint flakes more or less worked (chipped), said to have been discovered in the Miocene strata, have been adduced as indicative of Man's existence there at that still earlier period ; but some cautious geologists and archæologists have declined to accept the evidence.

We have now, however, to notice the finding of artificially prepared flint flakes, of human manufacture, in a Miocene formation of Further India.

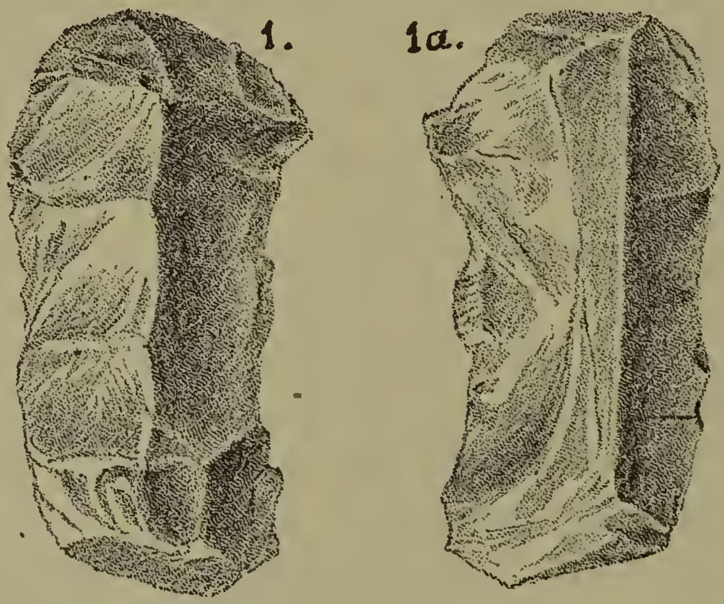
Lately, when mapping the Yenangyoung Oilfield, in Burma, Dr. Noetling was interested in collecting remains of Vertebrate animals, particularly in a ferruginous conglomerate upwards of 10 feet thick, persistent as a dull-red band across the ravines and hills. This rock, lying beneath 4,620 feet of Pliocene strata, contains numerous fossil bones, especially of *Rhinoceros perimensis* and *Hippotherium*

[1] "On the Occurrence of Chipped (?) Flints in the Upper Miocene of Burma." By Dr. Fritz Noetling, F.G.S., Palæontologist, Geological Survey of India. *Records of the Geological Survey of India*, vol. xxvii., part 3, 1894, pp. 101–103, with a plate [marked I.].

[2] See NATURAL SCIENCE, October, 1894, page 269, for special references to Professor Prestwich's several memoirs on this subject.

[*Hipparion*] *antelopinum*. When stooping for a tooth of the latter, Dr. Noetling found some chipped flint flakes, partly embedded in and projecting from the conglomerate; and close to the fossil tooth was one in particular, which he describes and figures (Figs. 1, 1*a*, and 1*b*). Figs. 1 and 1*a* are here reproduced.[3] About a dozen other flint flakes were also found in the rock. Some appear to have been like ordinary flattish "ridge-flakes," and up to about 40 mm. in length; some were smaller and more or less triangular; four of these last are figured (Figs. 2, 3, 4, and 5), each in three aspects.

The chief and largest of the flint flakes, thus fortunately observed and carefully extracted by a trustworthy observer, is elongate-oblong in shape, 45 mm. long by 20 mm. wide; rounded by chipping at one end, obliquely truncate at the other, and ridged on both faces; side-edges thin and sharp, almost parallel. It is sharply ridged on one face (Fig. 1) by the removal of somewhat symmetrical flakes struck off at right angles to the edges and the ridge. The other face (Fig. 1*a*) is bluntly ridged, a narrow portion of the original flake-face remaining


1. 1a.

along the middle, from which the sides slope more regularly on one side than the other; one edge having been more irregularly chipped, or, perhaps, subsequently broken, into a ragged and slightly concave outline.

There is no doubt of this being an artificially-dressed flake of flint, actually dug out by an experienced geologist of the Indian Geological Survey, or of the ferruginous conglomerate that contains it and other dressed flakes, belonging to the Yenangyoung Tertiaries of Burma, and to be regarded either as an Upper Miocene bed, that is, of the latest Miocene age, or, at the least, of the earliest Pliocene.

I.—The surface of India has, of course, yielded numerous flakes of agate and flint that have been manipulated by Man, as well as many stone implements of better workmanship. For recorded instances, see Evans's *Stone Implements*, etc., 1872, pp. 79, 103, 115,

[3] Fig. 1*b*, intended for an edge view, is apparently unsatisfactory in its perspective.

214, 249, 259, etc., and the *Manual of the Geology of India*, 1879, vol. i., pp. 440–442.

II.—The gravels and other materials drifted and arranged by rivers in remote times, when the levels of the country differed from those of the present time, and necessarily long before the rivers had settled down in the valley which they now drain, have frequently been found to contain other chipped flakes and rude tools of stone. Some of the latter, from the lateritic deposits in Madras and North Arcot, have been noticed by Foote and others in the *Quart. Journ. Geol. Soc.*, vol. xxiv., 1868, pp. 484, etc.; Evans's *Stone Implements*, etc., 1872, p. 570; *Memoirs Geol. Surv. India*, vol. x., 1873, pp. 43–58; *Manual of the Geology of India*, 1879, pp. 358, 369, and 441. A similar quartzite implement was found in the Narbada (Nerbudda) Valley by Mr. Hacket (*ibid.*, 1889, p. 386, pl. xxi., fig. 1; and in the second edition, 1893, p. 388). Others have been discovered in Assam, Bengal, Orissa, and elsewhere (*Manual*, 1879, p. 441). A "palæolithic celt" was found in the Punjab by Mr. W. Theobald (*Records*, etc., vol. xiii., part 3, 1880, p. 176).

Large cores and flakes from the bed of the River Indus, near Sikkarpoor or Sukkur, in Upper Sind, were found by Lieut. Twemlow 3 or 4 feet below the river-bed, in excavations made for some canal work. These consisted of the cream-coloured flint of the Nummulitic Limestone. Some of the cores were described and figured by Mr. (now Sir) John Evans in the *Geol. Mag.*, vol. iii., 1866, p. 433, pl. xvi.; and a note on the locality was given in the *Geol. Mag.*, vol. iv., 1867, by General Twemlow, who had some flakes also, one of which he gave me, together with a core. One of these cores from near Sukkur, on the Indus, is figured in the *Manual Geol. India*, vol. i., 1879, p. 442, pl. xxi., fig. 3; and flakes of this Nummulitic flint are there described as being common in Sind.

Small chalcedonic cores from the banks of the Mahanuddy River are mentioned in Evans's *Stone Implements*, etc., 1872, p. 21. So also small agate cores, found by Lieut. Irwing in the Nerbudda alluvium, are alluded to in *Geol. Mag.*, vol. iii., 1866, pp. 93 and 283. Chipped stone implements also were found on the affluents of the Kishna, by Foote,[4] in conglomerates on the Malprabha and its affluent the Beni-halla, in the South Mahratta country, where "fine, well-shaped, and mostly large-sized chipped quartzite implements" are very numerous wherever the red lateritic subsoil, which is older than the alluvium, is exposed.

A very definite ridge-flake of agate was found by Mr. Wynne in the ossiferous conglomerate on the Upper Godavery, near Pyton (Paitan) south of Arunzabad. It is noticed in the *Geol. Mag.*, vol. iii., 1866, pp. 93, 94, and 283, and in the *Records Geol. Survey India*, vol. i., part 3, 1868, pp. 61–65, with a plate. It is also mentioned and illustrated

Memoirs Geol. Survey India, vol. xii., part i., 1876, p. 241.

in the *Manual Geol. India*, 1879, pp. 389 and 441, pl. xxi., figs. 2, 2*a*. A good general note on the worked flakes and stone implements found in India is given at pp. 440–442 of the *Manual*, 1879, with references up to date.

Of course some of the occurrences of flakes and cores above-mentioned may be merely prehistoric, but others are of Pleistocene and some probably of Pliocene age.

Certainly the many stone implements and chipped flakes found in Southern India at different elevations appear to prove that great changes in the physical geography of the Indian peninsula have taken place since the time when the implement-makers first inhabited the country.[5] These terrestrial movements and consequent alteration of surface-conditions were mostly Post-Tertiary, but some probably took place in the Pliocene or late Tertiary times, when such disturbances appear to have been frequent and energetic.

III.—Dr. Noetling's discovery (see above, p. 346) takes us back to still earlier times, for he has good reason to regard the rock from which he took the specimen that we have described above from his notes and figures as of Miocene age. With a philosophic caution he gives the benefit of any doubt in this case to the early Pliocene (in the text) rather than to the latest Miocene (as in the title of the paper). We note that he throws an unnecessary doubt on the word "chipped" in the title of his paper, though there can be no doubt as to the artificial dressing of the flake. So also, in spite of his other doubt, the "Miocene" of Burma with its *Hippotherium* (*Hipparion*) may well belong to that geological stage, although by "homotaxis" (that is, the mere local succession of certain fossils) it might be supposed to be either older or younger, if its organisms had not been really contemporaneous, but localised by migration at unequal times—that is, at different periods. The *Hipparion*, however, one of the predecessors of the Horse, had powers of progression in and along the great Asiatico-European continent similar to those the Horse is known to have had; and its fossilised remains, therefore, characterise deposits in which they occur in France, Germany, Greece, Northern India, and Burma, not only as homotaxeous, but as being really *contemporaneous*. If the equine animal, so the then existing Man would be really of the same age as the Miocene fossils and the associated manufactured flint flakes of France (?) and India.

Long ago Dr. Hugh Falconer expressed his opinion that the remains of Man would be found in the Tertiary strata of India. The researches in the Sewaliks and elsewhere by Colonel Cautley and himself had caused them to entertain that idea; and in 1865 Dr. Falconer stated that more than twenty-five years previously they had shadowed out this speculation in a communication to the Geological Society of London, and that nothing had occurred to

[5] *Quart. Journ. Geol. Soc.*, vol. xxiv., 1868, p. 484.

invalidate it since.[6] Indeed he held it strengthened, for at page 388 he stated: "Here then was clear evidence, physical and organic, that the present order of things had set in from a very remote period in India. Every condition was suited to the requirements of Man; the lower animals which approach him nearest in physical structure were already numerous; and the wild stocks from which he trains races to bear his yoke in domesticity were established; why then, in the light of a natural enquiry, might not the human race have made its appearance at that time in the same region?"

Greatly would our old friend, this energetic worker and philosophic thinker, have welcomed such evidence of human workmanship in Miocene times, as is shown in Dr. Noetling's discovery. The late Dr. T. Oldham, fully appreciating Dr. Falconer's broad views and acute observations on the remote antiquity of Man in India, entered fully into the subject in the *Record Geol. Survey India*, vol i., 1868, pp. 66–69, and transcribed from his memoirs very important portions bearing upon it.

T. Rupert Jones.

[6] See *Quart. Journ. Geol. Soc.*, vol. xxi., 1865, p. 386.

III.

The Wing of Archæopteryx.[1]

Part I.

ARCHÆOPTERYX, as is well-known, is the oldest bird hitherto discovered, and forms one of the most interesting and remarkable fossils from many points of view. Thus far, only two examples are known, one being in the British Museum, the other in the Natural History Museum of Berlin. Both were obtained from the Solenhofen Slate (Lower Kimmeridgian) of Eichstadt, in Bavaria, though an interval of some years intervened between the discovery of the two fossils. For the present we are concerned with the wing only of the bird, and since this organ has been most wonderfully preserved in the Berlin example, all that I have to say in the first half of my paper will have reference to the second specimen.

Readers of Natural Science will remember that Dr. Hurst contributed to the pages of this Journal for October, 1893, an essay entitled " The Digits in a Bird's Wing: A Study in the Origin and Multiplication of Errors." He chose, as a case in point, the wing of Archæopteryx, and it would seem that his choice was an unfortunate one. The object of the present paper is to try and show that the result of that essay was but to contribute another to the already vast pile of errors Dr. Hurst had set himself to undermine. At the time this paper was read we met, so to speak, on equal terms, inasmuch as neither of us had ever seen the actual specimen. Since then, however, Dr. Hurst has made a pilgrimage to the Berlin Museum, and, therefore, has had the advantage of examining the fossil itself. My own information has been obtained from a study of photographs and the literature of the subject; while my investigations have been carried on in the Department of Comparative Anatomy of the University Museum, Oxford, and were undertaken at the instance of Professor E. Ray Lankester, who desired me to make a model of the wing, to be exhibited in the museum. This has now been done, and I should like here to record my grateful thanks for kind help and advice during the work of restoration.

In devoting the whole of the first part of the paper to a discussion of the wing itself, I trust I shall not seem to be wasting

no!

[1] Read at the British Association, Oxford, 1894.

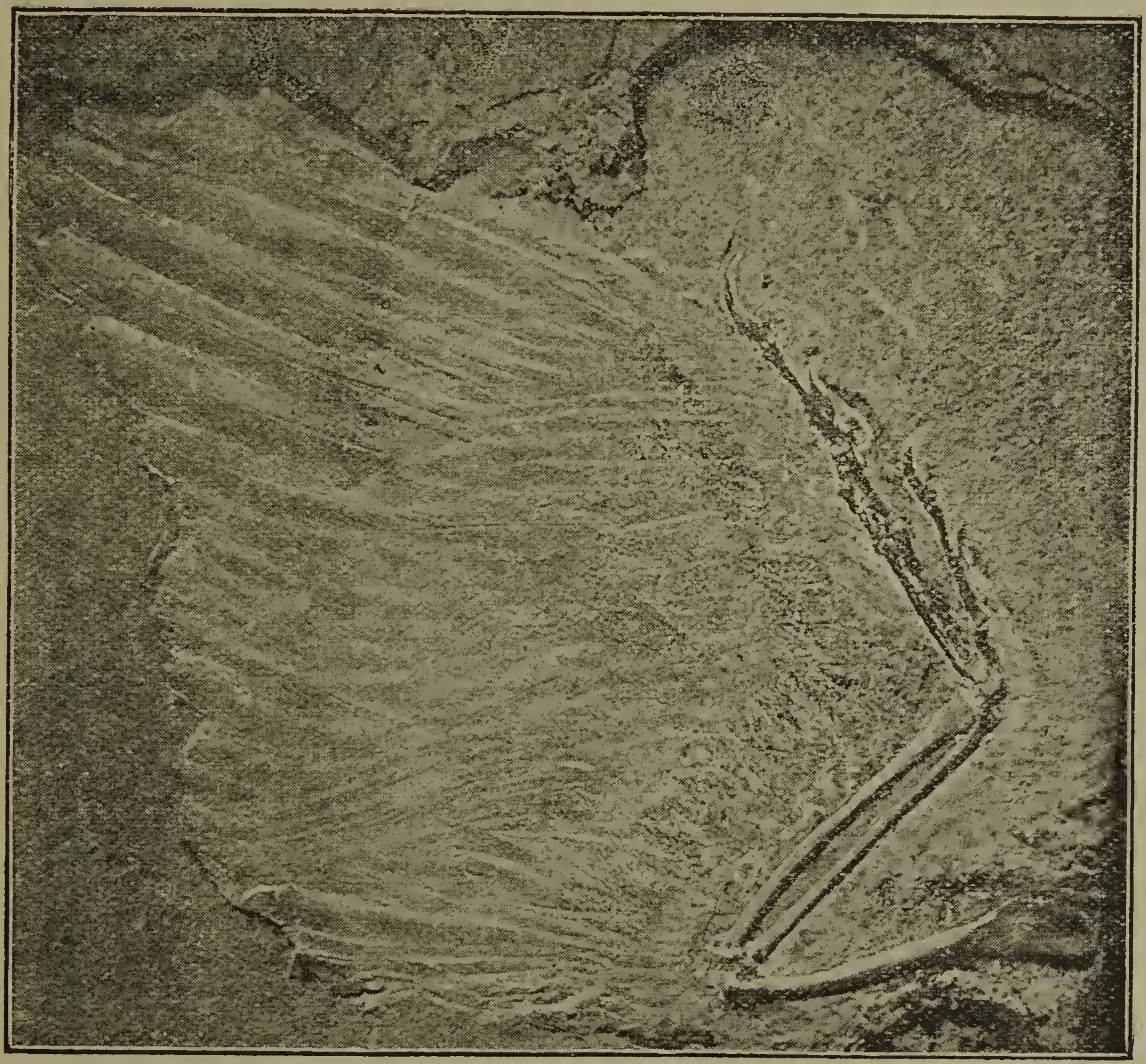


PLATE I.

Photograph of Left Wing of Archæopteryx, from the specimen in the Natural History Museum, Berlin.

[From article by Dr. C. H. Hurst, Natural Science, October, 1893.]

space; such a course seems to me imperative, if anything like an intelligible idea of what follows is to be gained. Throughout I have, whenever possible, compared the various points of importance with similar points in the wing of living birds, by which means we shall, I trust, grasp more thoroughly than heretofore the wonderful similarity between the wing of yesterday and that of to-day. In the second part, I hope to consider the various restorations which have appeared since the first discovery of the fossil.

The fore-limb of the higher vertebrates is pentadactyle, or five-fingered; thus, then, the manus of the modern bird is deficient in the number of its digits. This deficiency has come about during the specialisation of the organ for the purposes of flight; it became an interesting problem, therefore, to find out the lines along which this reduction has taken place—that is to say, were the two missing digits lost simultaneously or one at a time? and perhaps more important still, at what stage in the development of modern birds did they disappear, and which of the five do the three remaining digits represent? Hitherto, it has been taken for granted that the digits in the wing of existing birds correspond to the pollex, index, and medius, or, in other words, to nos. I., II., III. of the normal pentadactyle manus. This question has lately been raised by Dr. Hurst in the pages of this Journal, and will be discussed at length later.

With the component parts of a bird's wing my readers are, of course, all familiar; but I would just remind them that in the adult bird there are but two free carpal or wrist-bones, and these belong to the first or proximal row, while those of the second or distal row have become anchylosed or fused together into one mass, blending at the same time with the bones of the metacarpus or hand. In the embryo, however, we may have as many as seven distinct carpals, and the metacarpal bones are free. Such a stage as this we assume to represent a sometime adult form, and it is not, I think, beyond the pale of possibility that fossils showing some such arrangement may be discovered.

In how far the oldest known fossil bird, Archæopteryx, contributes towards the chain of gradations which must have existed we unfortunately cannot tell, inasmuch as, though there can be little doubt that the number of digits did not exceed three, and that the metacarpals were not fused together, the carpus is not sufficiently well-preserved to enable us to say anything definite. From the photographs which have been lent me, I fancy I can make out, in one, a large sub-crescentic mass, opposed to the base of the metacarpals, but quite separate from them, and representing a fused row of distal carpals, and, very imperfectly, a radial and an ulnar carpal, with an intermedium, closely applied to the radiale, between them. In the photograph accompanying this paper, which was taken in a different light, the distal carpal would seem to have fused with the metacarpal bones II. and III., but does not extend to I. Hence, how much is due to

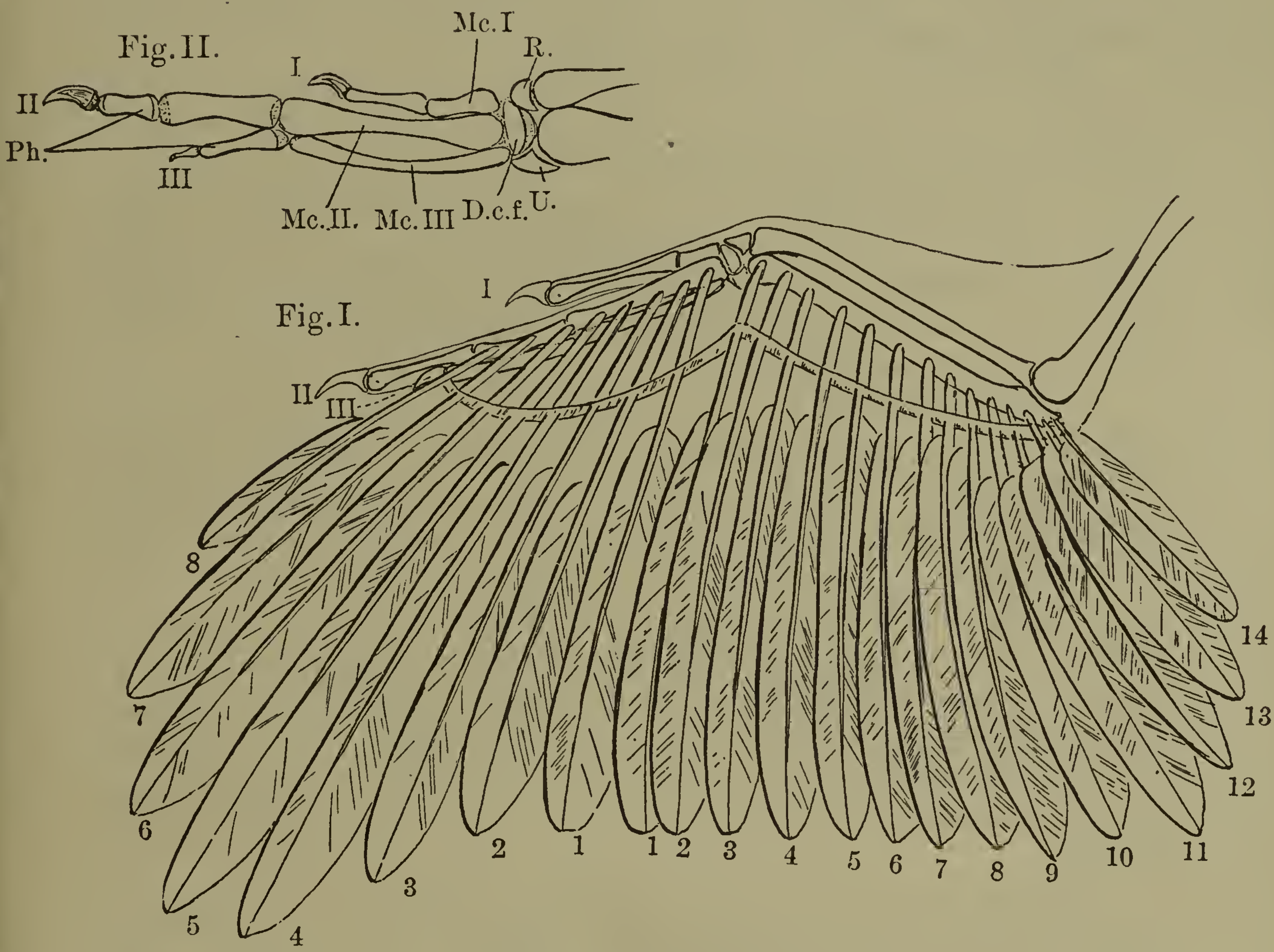


PLATE II.

FIG. I.—Restoration of the wing of Archæopteryx, showing the probable distribution of the remiges or quill feathers.

1–8.—Metacarpo-digital remiges or primaries.

1–14.—Cubital remiges or secondaries.

I., II., III. = Digits I., II., III.

The band running along the bases of the quills represents the tendon described on p. 356.

FIG. II.—Portion of the wing of a half-grown Ostrich (after Parker) showing the component parts of a bird's manus, and illustrating the similarity between the manus of Archæopteryx as seen on the Berlin slab with that of existing birds.

R.—Radiale. U.—Ulnare. Mc. I., II., III.—Metacarpals I., II., III. Ph.—Phalanges.

shadows and how much is real, is a matter upon which Dr. Hurst will be much more qualified to speak than I. Up to the present, all who have described the fossil seem to agree that only one free carpal can be certainly made out, and that this probably corresponds to the radiale.

Whichever of the two possible arrangements of the carpus is correct, the wing of Archæopteryx resembles in a remarkable degree that of existing birds; this fact will be seen at a glance if the reader will turn to Pl. II., Fig. II., where he will find for comparison the figure of the wing of a half-grown ostrich.

The differences, then, I think, between the wing of Archæopteryx and that of existing birds, are rather those of degree than of kind, such as the fusion of the metacarpals, and the reduction in the number of the phalanges of the third digit, and the little platforms of bone thrown out from the posterior margin of the phalanges of the II. digit; these are merely improvements on the original pattern, so to speak. The metacarpal bones of Archæopteryx were evidently quite as stout as those of modern birds; that of the III. digit is even stouter.

In the possession of claws, Archæopteryx agrees with existing birds, in that we might enumerate many species in which claws are present on digits I. and II., and in at least one instance—that of a young ostrich—on III. also. The function of these claws we shall discuss later.

The resemblances between the wing of Archæopteryx and that of modern birds, as I have just said, are greater than the differences; or to be more exact, the bones of the arm, forearm, and hand of Archæopteryx correspond so nearly with those of modern birds, that there seems no reason for suspecting that there existed in Archæopteryx any structures—such as additional digits—other than those seen on the slab.

Judging from the photograph, the wing-feathers of Archæopteryx seem, relatively to the size of the bird, quite as well adapted to the purposes of flight as those of modern birds; and we may infer, therefore, they were arranged and attached to the skeleton in a very similar manner. I doubt not but that, to many of my readers, the precise relations between the skeleton and the supported feathers have hitherto been regarded as details, not necessary, or of any value whatsoever to them in the "struggle for existence," and hence, they have been entirely ignored. Let me briefly sketch the facts. The wing-feathers are divided into (A) remiges or quills, and (B) tectrices or coverts. The remiges are the longest and strongest feathers in the wing, and run along the posterior (postaxial) margin of the forearm and hand. They are divided into (α) primaries (metacarpo-digitals), and (β) secondaries (cubitals). The primaries rarely exceed eleven in number, and never fall below nine, while the secondaries vary considerably. The primaries are distributed along the manus in a definite order; thus, save in rare exceptions,

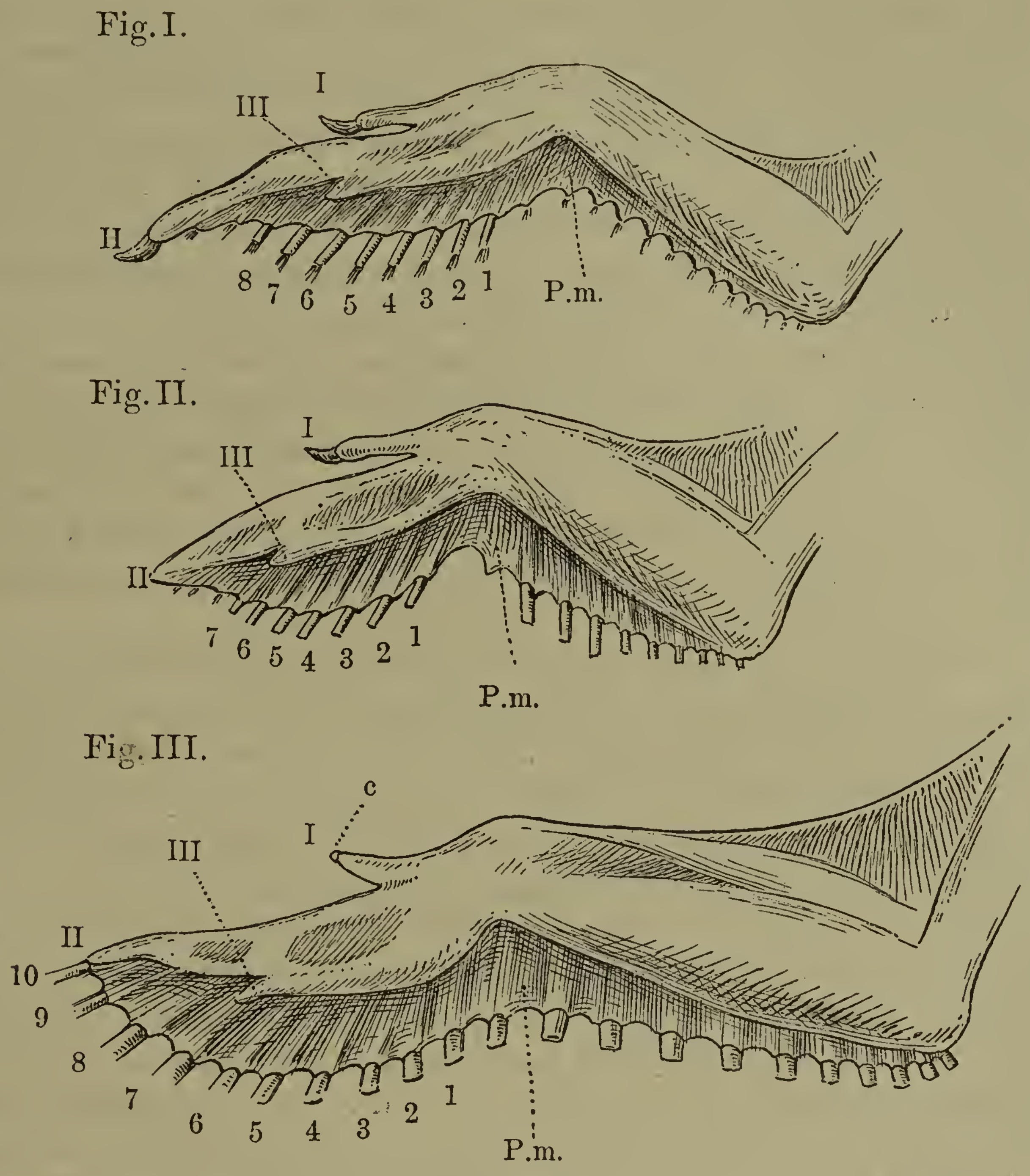


PLATE III.

FIG. I.—The right wing of a nestling *Opisthocomus cristatus*, ventral view, showing how that the second digit (II.) is produced beyond the ala membrana (P.m.), and that the development of remiges 8–10 has been arrested, so as not to interfere with the freedom of the long finger when climbing.

FIG. II.—The right wing of a nestling of the Common Fowl (*Gallus bankiva*), an ally of *Opisthocomus*. Owing to the exchange of an arboreal for a terrestrial habit, the manus is gradually shortening. The pollex only retains the claw. The II. digit projects but slightly beyond the ala membrana; but the development of the most distal remiges is still arrested.

FIG. III.—The right wing of an adult *Opisthocomus cristatus*, ventral view. Contrast with that of the young bird, and the manus is now shorter than the forearm, the "pollex" or "thumb" is considerably reduced, as is also digit II. The claw may be seen on the pollex as a slight wart (c).

1–10.—Metacarpo-digital remiges, or quill feathers. P.m.—Ala membrana or posterior wing membrane. c.—Claw.

the metacarpal bones support 1—6 (metacarpals) the single phalanx of digit III. bears no. 7 (addigital); 8, 9 are attached to phalanx I. of digit II. (middigitals), and the remaining feathers to the antepenultimate phalanx of digit II. (predigital). This is the normal arrangement as we find it to-day. Bands of muscle and tendon bind these feathers together, and to the skeleton, so firmly that a large amount of force is necessary to remove any one of them. The whole of this knitting together is completed by a strong tendon running along the posterior (postaxial) margin of the wing some distance down the quill, from the elbow to the tip of the longest finger, the quills appearing much as though they had been pushed through it, so as to ensure their preserving the right distance apart, and all opening and closing in unison. The quills of the hand are set rather closely together, so much so, that they nearly touch one another at the base, but not quite; thus a series of troughs is formed, and in these lie the bases of the strong, stiff, major coverts which therefore serve as an additional source of strength, binding all into a compact whole.

Turning to the restoration, it will be seen that I have assumed some such arrangement to have obtained in Archæopteryx, as that which has just been described. Indeed, the number and position of the digits, the proportion, texture, and position of the feathers, are so clearly indicated on the slab, that I should rather be said to have copied than "restored" this wing, using that of living birds as a guide to the details. But let the reader turn to the photograph (Pl. I.) and judge for himself. First of all, I would suggest that he should begin by "setting" digit III., which has evidently been dislocated. Then, if the line of the shafts of the quills be carried up toward the skeleton, that of the outermost and shortest will be found to lead to the tip of digit III. running alongside the claw; tracing up the others in like manner, he will find that he has room proximally for some two more feathers, which I have taken the liberty of adding; the fact that these are not indicated on the slab is probably due to the cubital remiges overlapping them.

We have now to consider the question as to whether the actual feathers (or their equivalents from infiltration) are preserved on the slab, or whether what is left is but the impression of the ventral surface; either of these two views is possible. I do not remember to have seen this point raised before, but the general impression among my friends seems to be that, what we have presented to us is the ventral surface. If this is the case, as my colleague Mr. E. S. Goodrich has pointed out to me, not feathers but only their impression is left on the slab.

I should be delighted if it could be proved beyond doubt that this latter view is correct, for we should be more easily able to understand the long, semiplumous covert feathers partly concealing the remiges, inasmuch as the coverts of the under surface are often of precisely this texture in modern birds. Again, this would account for the

fact that no trace of the base of the quills can be seen, as well as explain the facts noticed by Dr. Hurst when examining the fossil. The notes which he has kindly sent me contain the following passage: "Feathers along outer side of ulna not cut away but apparently shifted (through decay of muscles, etc.) so as to lie in same horizon as axis of ulna in greater part, but distally, somewhat lower and distinctly beneath the three visible digits."

Supposing, however, that we are looking at the dorsal surface; then it is not quite easy to see how it is that no trace has been left of the base of the quills resting on the skeleton, for I am told these have not been cleared away to expose the bones. By way of complicating matters, we have to explain the semiplumous feathers before referred to. I think it is evident that these cannot be the homologues of the major coverts previously mentioned. In the first place, chiefly because they not only cross the quills at a considerable angle, but extend beyond them to the tip of the II. digit. They must, then, represent some one or other of the series above the major coverts. Unlike the quills, which would scarcely seem to differ from those of modern birds, these coverts, in their length and texture, depart from the normal habit of dorsal coverts, and resemble rather the ornamental plumes such as decorate the wings of *Cotinga pompadora*.

The crucial test, to my mind, to decide which of the two surfaces we are dealing with, is that of the overlap of the feathers; thus, if we are looking at the dorsal surface of the wing, then the feathers should have a distal overlap, *i.e.*, the free edges of all the feathers will be outwards; if at the ventral, then we ought only to see one broad vane, not two as in the upper surface, and that vane the inner, so that its free edge will be proximal or inwards. Whether or not I have a defective vision remains to be proved, but I feel persuaded that we have the dorsal surface presented to us, and not the ventral. If the reader will note the direction of the light from which the photograph was taken he will find that the distal vane of every feather is lit up, while the proximal vane is in shadow; further, it will be seen that mud has drifted in between some of the feathers, *e.g.*, 5; which, reflecting the light, sharply defines the outer vane of one feather from the proximal immediately under it.

In order, however, to prove this point if possible, I have taken a cast of a sparrow hawk's wing. This I propose to have photographed, illuminating it from the same direction as that of the Berlin photograph, when we shall, I imagine, gain precisely the same effects of light and shade.

Now let us consider the probable function of the claws and power of flight in Archæopteryx.

In fairness to myself, I must beg the reader to follow me a short space off the beaten track, in order that we may discuss what seems a rather important piece of evidence in support of the theory I wish to propound.

Some time since I discovered that in the nestling of the common fowl, out of the ten developing remiges, the growth of the three nearest the tip of the wing was completely arrested, after a certain stage, until comparatively late in life.

Here was a mystery which seemed to admit of no interpretation. What could be the meaning of this extraordinary fact? The key to this riddle was obtained through the kindness of Dr. P. L. Sclater, who recently placed in my hands for investigation a series of nestlings of *Opisthocomus cristatus*, a very primitive and altogether remarkable gallinaceous bird. These were acquired, at the instance of Dr. Sclater, out of a grant from the Royal Society by Mr. Quelch, curator of the British Guiana Museum. For a most interesting account of the habits of these birds I must refer my readers to Mr. Quelch's valuable paper in the *Ibis* for 1890. With one of the many strange facts contained in this essay, however, we are immediately concerned, and that is the wonderful powers of climbing, and, stranger still, of swimming, that the nestling seems to be endowed with.

It would seem that the habits of this bird are arboreal in the strictest sense of the term. The young are, therefore, reared in the trees; but, apparently, instead of remaining in the nest until they are able to fly, they are in the habit, at a very early stage in their life, of climbing out of the nest,[2] it may be to gain a better coign of vantage whence to meet the parents returning with food, or to take refuge in more dense foliage to escape an enemy—sometimes the one and sometimes the other cause impels them to leave their home; in either case it is obvious that the chances of a fall are exceedingly probable. Now, as might have been expected, these youngsters are particularly capable of taking care of themselves, possessing not only enormous feet fitted for grasping—like the parents—but they have powerful auxiliaries in the shape of the beak, which is used much as is that of a parrot in climbing, and wings which are armed with large claws on the first and second digits. On turning to Fig. I., Pl. III., it will be seen that the general form of the wing renders such a mode of locomotion quite probable, and may therefore be said to entirely corrobate Mr. Quelch's statements. The hand is considerably longer than the forearm, the pollex or thumb extends beyond the level of the tip of the III. digit, and is provided with a large claw; the II. digit, with an equally large claw, is produced beyond the fold of skin running along the posterior border of the wing, which encloses the base of the quills. Of these, it will be noticed, only 1—8 have extended any distance beyond the wing-fold just mentioned, so that a long free finger-tip is left. As the bird grows and the feathers develop, the proximal ones grow faster than the distal, so as not to impede the freedom of the hand in

[2] This seems to be always placed over the water, and should the young by any accident fall in they save themselves by swimming; if pursued, they dive!!

climbing; but as soon as the proximal feathers have increased sufficiently to serve to break the force of a fall, should such occur, the remaining distal feathers begin to develop; at the same time, the hand begins to shorten, till, as will be seen in Fig. III., in the adult the hand has come to be *shorter* than the forearm, the claws have disappeared, the thumb no longer extends to the level of the III. digit, nor does the II. project beyond the posterior wing-fold, the bird now being able to move from one place to another by flight instead of by climbing. In the course of development, I imagine the species has managed to shorten the hand and lose the claws as soon after they ceased to be of assistance as possible; but could we discover a yet more primitive form it is probable that we should find that the claws and long hand were retained throughout life.

? absolutely or relatively?

! ·X·

The common fowl, being a more recently-evolved form which has exchanged its arboreal for a terrestrial habit, is gradually eliminating this phase in the life-history. The hand of the nestling is still longer than the forearm, though not much; but the II. digit is scarcely produced beyond the wing membrane and the claw has been lost though present in the embryo, the pollex has retained the claw, and extends just up to the III. digit. The arrested development of the quills, however, still persists, and apparently for the same period as that in *Opisthocomus*, though it has long since ceased to have any function.

?

If we are to believe that birds have been derived from an arboreal ancestor, then *Opisthocomus* proves to be a bird of most exceptional interest, for it is probable that the peculiar habits of the nestling may be a survival of an order of things handed down from the very dawn of avian development.

on what ground?

If these speculations have any value, will it seem too great a demand upon the imagination to suggest that the claws in Archæopteryx were of prime importance only during the *nestling* period, and that their presence in the adult may be rather due to the fact that they had not "had time," so to speak, to erase them after they ceased to be functional, as *Opisthocomus* has succeeded in doing to-day? Possibly they may have been used by the adult on rare occasions, but surely it is doubtful.

Yes. It will seem too great.

The view that my readers take with regard to this matter will depend entirely upon their conception of the power of flight of Archæopteryx. Generally speaking, it is probable that it has been considerably underrated. Perhaps undue anxiety to enhance the value of Archæopteryx as connecting link between the Reptiles and Birds has done not a little to warp the views of many of those who have expressed an opinion on the subject. Usually, if we ask for some sort of estimate of the flight of this ancient bird, we are referred to flying lizards and flying squirrels—unsatisfactory comparisons.

Judging from the relative size of the wing as compared with the

·X· this means only a form more closely resembling Pycraft's imaginary primitive form!

body, we might justly have expected something more worthy of the name of flight than this, though possibly it may not have been of a much higher order than that of *Opisthocomus*, which is said never to have been seen to take a more extended journey than that of a few yards at one time. The presence of the claws, too, has done much to injure its reputation as a flyer—a factor, as we have seen, which probably ought not to have been allowed to weigh at all.

W. P. Pycraft.

IV.

Further Notes upon the Organs of Arachnids.

IN an article contributed to NATURAL SCIENCE (vol. iii., p. 441) at the end of last year, and in some previous reviews therein mentioned, I endeavoured to trace the light thrown by recent researches upon the structure and relationships of the spiders and their allies. Several fresh memoirs upon the subject have appeared during the year now passing away, and some account of these also may prove of interest.

A question discussed in several of my previous reviews was whether lung-books or tracheal tubes are to be regarded as the older form of breathing organ among the arachnids. The majority of modern naturalists, following Professor Ray Lankester in considering *Limulus* a marine arachnid of ancient type, believe the lung-books of scorpions and spiders to have been formed by the inpushing or sinking of the gill-bearing limbs of a water-breathing ancestor, and the tracheal tubes found in the higher spiders and other arachnids to have been derived by the simplification of such lung-books. The opposite view is maintained by Mr. H. M. Bernard, who, considering *Limulus* a " specialised crustacean," sees in the tracheæ of arachnids the representatives of the original series of ventral setiparous glands in a hypothetical worm-like ancestor, and believes that lung-books are a new development from the tracheæ. In support of this idea Mr. Bernard has in several recent communications (1, 2, 3) announced the discovery in the Chernetida (false-scorpions) of a series of paired vestigial stigmata on all the abdominal segments behind the fourth (the functional stigmata leading to the tracheal tubes being found on the third and fourth abdominal segments). Unfortunately, however, this observation has been severely called in question by Dr. H. J. Hansen, who, in a discursive paper of considerable interest (4), dismisses the " vestigial stigmata " as nothing but lyriform organs (*see* NAT. SCI., vol. i., p. 525), of which he finds not a pair only, but two pairs on each abdominal segment. Mr. Bernard, in his latest paper on the subject (5), returns undaunted to the charge, and expresses his conviction that the objects under discussion are not lyriform organs. He admits, however, that there are two pairs of them on some segments, and suggests that if one series represent vanished stigmata, the other may be the vestiges of former spinning or cement glands.

But while the facts are still a matter for controversy, the construction of phylogenetic theories can but be speculative.

Another order of arachnids—the Pedipalpida, or scorpion-spiders—is, however, brought forward by Mr. Bernard in this last paper to confirm his opinion. Examining dried specimens of Thelyphonidæ so that the ventral surface of the abdomen reflects the light, he finds, outside the muscle impressions from the fifth to the eighth segment, a paired series of scars, supposed to represent vestigial stigmata, and of areas supposed to represent the flattened vestiges of vanished limbs. Hence he concludes that the ancestor of the Arachnida possessed a pair of limbs and a pair of stigmata on every segment. That a very remote ancestral form of the group possessed a long series of paired limbs, functioning as breathing organs, is quite possible; but the fact that in primitive living terrestrial arachnids (the scorpions), as well as in both living and extinct aquatic arachnids (*Limulus* and the Eurypterida), no breathing organs occur behind the sixth abdominal segment, renders it highly probable that in the immediate ancestors of the class the series had already become so far reduced.

In his researches upon the Chernetida (3), Mr. Bernard has paid much attention to the glands. He confirms Croneberg's statement that the spinning-glands are situated in the cephalothorax, wherein they occupy a large space dorsally, and that their ducts open behind the tips of the movable fingers of the cheliceræ. The peculiar comb-like structures on these fingers, generally known as flagella, are, therefore, probably used in working up the silk. The glands, opening by median papillæ upon the second and third abdominal segments, which were thought by some observers to produce silk, are believed by Mr. Bernard to secrete cement for fastening the eggs together. The coxal glands have openings on the hinder face of the coxæ of the third pair of legs.

Opening on the first abdominal segment, beneath the genital operculum in Chernetids, are a pair of curved conical tubes, with transversely folded walls, known as "ram's-horn" organs. These are said to occur only in the male, and being sometimes found evaginated, were believed by Croneberg and Menge to have a reproductive function, although the former considered them homologous with the tracheal tubes. Mr. Bernard has found these structures in one of his specimens, and observing that they are surrounded with air-chambers, concludes that their function is respiratory. He naturally regards them as homologous with the tracheal tubes, and expresses his belief that they represent the starting-point from which tracheal tubes or tufts and lung-books have been elaborated. He would thus find here another argument for the primitive nature of tracheæ with respect to lung-books.

In a former review in Natural Science (vol. ii., p. 449) I mentioned some well-known facts of comparative anatomy and

palæontology which strongly support the generally-received view that lung-books are the more primitive. A recent investigation (6) by Mr. O. L. Simmons into the development of the breathing organs of spiders supplies valuable embryological evidence upon the same side. This observer finds that in species of *Theridion* and *Agelena* the lung-plates arise upon the hinder surface of abdominal limbs in exact correspondence with the origin of the gill-leaves of *Limulus*, and that the appendage simply sinks into the abdomen without inversion or other complication, exactly in the way lately suggested by Dr. Kingsley, and figured in NATURAL SCIENCE (vol. iii., p. 442). In tracing the development of the tracheæ (which in the higher spiders replace the hinder pair of lung-books), Mr. Simmons finds a stage showing folds indicative of the lung-plates, which vanish later. We have here, then, another link in the strong chain of evidence that among arachnids lung-books preceded tracheal tubes, and that they represent the gill-bearing appendages of aquatic ancestors.

Two interesting cases of virgin reproduction among arachnids have recently been noted. Hitherto, the occurrence of parthenogenesis has hardly been known in the class, though instances among the other two great arthropod classes—the crustaceans and insects—are sufficiently familiar. Now, however, M. Trouessart has (7) brought to light a case among the mites, and Herr Damin (8) one among spiders. The former observer has studied a species of sarcoptid (*Syringobia*) which infests the wing-feathers of the Redshank (*Totanus calidris*). While the bird is on its migration between its northern summer breeding haunts and its southern winter resorts, two kinds of colonies of the mite are found within the quills. The normal colony consists of males and females in their adult and preparatory stages. In the parthenogenetic colony are found specially modified females, incapable of impregnation but producing fertile eggs; very rarely, a few abortive males are present. During the bird's sojourn in the south the mites live freely upon the barbs of the feathers; when the time for migration draws near, three or four in the nymph stage of development enter the quill. According to M. Trouessart, if both sexes be represented among these, a normal colony will result, but if all be females they will become capable of virgin reproduction—a truly remarkable adaptation to their confined mode of life during the period of migration.

The parthenogenetic spider observed by Herr Damin was a female of *Filistata testacea*, a species common in the south of Europe. Taken while still immature, this spider underwent two moults, and then, never having been in company with a male, spun a cocoon and laid eggs, which in due time hatched, producing healthy young. Some time after this, strangely enough, another casting of the skin took place. In connection with his observation, Herr Damin notices that while females of *Filistata* are very common, the male is exceedingly scarce.

The memoir of Dr. Hansen (4), already mentioned, adds considerably to our knowledge of sense-organs in arachnids, supplementing in many respects M. Gaubert's researches, already reviewed in NATURAL SCIENCE (vol. i., p. 525). Lyriform organs were not observed by M. Gaubert in scorpions or in Solifugida. Dr. Hansen has found them on most of the joints of all the walking legs, and on two joints of the palps, in both *Scorpio* and *Buthus;* and also on the middle finger of the cheliceræ of various genera of solifugids. He has noted them, too, in various regions of arachnids of other orders where they had not been seen by M. Gaubert. In a series of ridges and furrows upon the inner face of each chelicera in solfugids, Dr. Hansen believes he has discovered a peculiar stridulating organ. He describes and figures many interesting varieties of tactile hairs and other sense-organs. A specially interesting discovery is that of two air-stigmata, one at the proximal and one at the distal end of each of the eight tibiæ of the legs of *Phalangium* and some harvestmen of other genera. The existence of such stigmata on limbs is claimed by Dr. Hansen as unique among arthropods, and seems a special adaptation to the excessive length of leg characteristic of the Phalangiidæ. In the other families of the order, whose legs are relatively shorter, these stigmata appear to be absent.

In his publications on arachnid morphology, Mr. Bernard mentions an extensive research upon the Solifugida which he has in progress. A study of the animals of this order, with their imperfectly fused anterior segments and other archaic characters, cannot fail to throw additional light upon the points now under dispute. Mr. Bernard has already issued several preliminary notes, the most interesting of which (9) announces the discovery of lateral eyes in these animals. Hitherto the two central eyes only had been observed. Two lateral eyes have now been found by Mr. Bernard on either side of the cephalothorax; owing to the great development of the cheliceræ, they seem to have travelled from the dorsal to the lateral surface. They appear to be degenerating, though in some cases a ganglion and retinal cells can be observed.

REFERENCES.

1. **Bernard, H. M.**—"Additional Notes on the Origin of Tracheæ from Setiparous Glands." *Ann. Mag. Nat. Hist.* (6), vol. xi., 1893, pp. 24–28.
2. —————— "The Stigmata of the Arachnida as a Clue to their Ancestry." *Nature*, vol. xlix., 1893, p. 68.
3. —————— "Notes on the Chernetidæ, with special reference to the Vestigial Stigmata, and to a New Form of Trachea." *Journ. Linn. Soc.* (*Zool.*), vol. xxiv., 1893, pp. 410–430, pls. 31, 32.
4. **Hansen, H. J.**—"Organs and Characters in different Orders of Arachnids." *Entomol. Meddelelser*, 1893, pp. 137–252, pls. ii.–v.
5. **Bernard, H. M.**—"Vestigial Stigmata in the Arachnida." *A.M.N.H.* (6), vol. xiv., 1894, pp. 149–153.

6. **Simmons, D. L.**—"Development of the Lungs of Spiders." *Am. Journ. Sci.*, vol. xlviii., 1894, pp. 119–128, pl. viii. *A.M.N.H. t.c.*, pp. 210–221, pl. vi.

7. **Trouessart, E.**—" Sur la Parthénogénése chez les Sarcoptides plumicoles." *Comptes Rendus*, vol. cxviii., 1894, pp. 1218–20.

8. **Damin, N.**—" Ueber Parthenogenesis bei Spinnen." *Verh. k.-k. Bot. Zool. Ges. Wien*, vol. xliii., 1893, pp. 204–6. (Tr. in *A.M.N.H.* (6), vol. xiv., 1894, pp. 26–29.)

9. **Bernard, H. M.**—"Lateral Eyes in the Galeodidæ." *A.M.N.H.* (6), vol. xiii., 1893, pp. 517–20.

GEO. H. CARPENTER.

V.

Anlagen.

WHEN one is studying the actual development of any organism, one sees, in microscopic sections, the beginnings of organs before these have assumed definite shape and form. In some cases, as in the case of the developing reproductive organs of many animals, these beginnings come into visibility first as three or four definite cells, clearly marked out from the cell-mass in which they lie, by difference in character which is directly visible, or by difference in character apparent only by difference of reaction to certain stains. In other cases, perhaps in the majority of cases, the beginning of an organ or structure, so far as visibility to us goes, is not two or three separate cells, but a thickening of tissue resulting from the more active proliferation or multiplication of cells in some definite area. For instance, in many vertebrates the nervous system, as it first becomes visible under the microscope, is a broad band or plate distinguishable under the microscope by the relative opacity it causes along the dorsal surface of the embryo. At first the whole young embryo of an animal or plant is, in the majority of cases, a mass of cells indistinguishable from each other in appearance. At various points in this mass the various organs come up into visibility, successively or simultaneously, in definitely localised regions of the common mass of germinal cells.

For these first visible appearances of organs German writers use the word "Anlage," borrowing it from its familiar use as the word denoting the actual concrete beginning of some operation. Thus *die Anlage eines Gartens zu machen* is not to draw out the plan of a garden on paper, but to make the footpaths and flower-beds.

Starting from the objective and visible fact, that organs appear as visible proliferations or buds of cells in definite regions, biologists in many cases have carried the idea back to the invisible and theoretical. Some have supposed that the hereditary mass of the fertilised egg-cell itself is a mosaic of definitely-placed though invisible primitive beginnings of organs, and that the visible beginnings of these organs, in the embryonic mass that results from the division of the egg-cell, are due to the presence in such areas of the

invisible beginnings handed on from the egg-cell. For those who, like Weismann, take an extremely preformationist view of development, the imagined number of definite but invisible beginnings present in the egg-cell is very great, and these beginnings themselves are arranged in ordered hierarchies. But even Oscar Hertwig and many of the supporters of more epigenetic theories of development allude to the presence of definite physical beginnings in the structure of the egg-cell. Now the word used by Germans for these imagined particles of the hereditary mass is also "Anlage."

There is wanted, then, an English word to express these two meanings, to include both the visible beginnings of organs and the beginnings of these as imagined before they become visible. Everyone who has had to translate, into English, German writings dealing with embryology or with biological theory has met with the difficulty of translating "Anlage" at a very early stage of his work; and even in English writing which is not based on German originals the difficulty of expressing the idea has to be faced. In the search for an English bearer of the idea one must remember first that the idea is a concrete fact, not an abstract conception: what we want to express is a definite budding or proliferation of cells on the one hand; on the other, a definite particle of nuclear matter that in suitable conditions is able to control or cause the formation of the definite bud or proliferation of cells. English writers have made many different attempts to get hold of a suitable word. The first and most obvious word is "rudiment." But, as Professor Edward Mark has already pointed out in a preface to his translation of Oscar Hertwig's "Text-Book of Embryology," rudiment is quite unsuitable, because, both in English and German, precise writers have used it in the sense of an organ or structure which has been at one time more complicated that it is at present. But a reason that seems to be still more against its use as a rendering of the ideas connected with "Anlage" is that we use rudiment most commonly as a term denoting structures in adult organisms, whereas "Anlage" is invariably, or almost invariably, connected with embryonic structures. Such words as "origin," "beginning," and so forth, fail to express the concreteness of "Anlage," the fact that it refers almost invariably to objects that are either actually visible, or that have a corporeal existence although invisible to us. Occasionally, but very rarely, Weismann uses the word in a sense that has led Professor W. N. Parker to render it by "predisposition"; but Weismann's use of it in this sense is associated with a tendency of his, especially noticable in "The Germ-Plasm," to glide rather easily from the abstract to the concrete; in at least some of the cases where I have compared the translation with the original, it seems to me that his meaning would have been more clearly, if less invitingly, expressed by using a concrete English term for a word that in German is abstract only in a metaphorical sense.

"Basis" and "foundation" also are used by some writers; but these are the renderings in English of the German word "Grundlage," and express the structure upon which a superstructure is "reared," not the first stage of the superstructure itself. The word "forecast" has been suggested by several writers, among others by Mr. E. A. Minchin. It would have been a word very suitable in the cruder days of the theory of preformation; in these days it seems to imply too close a connection in form between the "Anlage" and the organ arising from the "Anlage." Professor Mark, who has discussed the difficulties of the matter, employs the word "fundament"; this is a neologism that few would have occasion to resent. I have myself employed the word "incipium," but that offends Latinity, and fundament, incipium, and primary constitutent alike fail to convey the definite idea that "Anlage" is a growing structure in a growing organism, or, when it is used of nuclear particles, a structure of which a chief property is to grow or cause growth. The purpose of the present note is to suggest as an English equivalent of "Anlage" a word that would be intelligible to all English and German and French biologists, and that would be understood easily by any educated reader. The word I propose is "blast," a bud or point of growth. The existing meanings of "blast" could cause no confusion, and the word would indicate equally well the visible beginnings of organs in the form of cells or cell-groups, and the hypothetical precursors of these as parts of the hereditary mass. The obvious existing precedent for the use of the word is the common acceptation of the terms "epiblast," "mesoblast," and "hypoblast" as indicating the three great groups of cells that are formed in the development of all the higher animals.

It may be objected to the term "blast" that it would be inconvenient, as the epiblast, hypoblast, and mesoblast contain the "Anlagen" or "blasts" of many organs and structures. But in this hypothetical objection there is contained what seems to me a possible advantage in the recognition of the term "blast" in the sense I suggest. The days are gone by for embryologists to see in the layers of cells known as the epiblast, hypoblast, and mesoblast simple structures of a homogeneous and equivalent nature throughout the animal groups, and there are many who would hesitate before the suggestion of homology between these groups, except in very closely-allied animals. In the case of the mesoblast, it has been recognised almost since the inception of the term that the group of cells is a composite group, containing derivatives from many sources, and containing different sets of derivatives in different animals. For a layer or group of cells that, in the vertebrates, includes a set of primitive structures, "Anlagen" or "blasts" so varied as the primitive segments, the epithelium of the cœlom, the sexual cells, the epithelium of the sexual ducts and kidneys, and the beginnings of the connective tissues, blood-vessels, and blood, the term "mesoblasts,"

"middle blasts," would be more logical and not less intelligible than the single term "mesoblast," with its misleading suggestion of homogeneity. In the case of the other layers, the compositeness of the groups of cells is less obvious, and equally there is less reason to suppose that there could arise any confusion were they to retain the simple designations "epiblast" and "hypoblast," while the word "blast" was used by itself in the sense I now suggest for it.

P. Chalmers Mitchell.

VI.

A Portable Zoological Station in Bohemia.[1]

SINCE the establishment of the commission for investigating the Natural History of Bohemia in 1864, it has ever been one of its chief objects to study the life of the lakes, ponds, and streams. So early as 1871 Dr. Anton Fritsch began to publish some of the results of his work in this direction in the *Archiv* of the commission ; and since that date much valuable research has been accomplished both by Dr. Fritsch and other authors. For nearly twenty years, however, the task could only be undertaken in a very unsystematic and spasmodic manner. There was no provision for making observations on fresh material on the spot. Dissections and microscopical work could not well be done in the open air, and it was impossible always to find a convenient and quiet cottage near the various pieces of water where the naturalist could temporarily take up his abode. Moreover, it was difficult to extend the period of collecting at any one spot, and to examine the fauna and flora under varying conditions. It therefore appeared to Dr. Fritsch, who was foremost in directing the undertaking, that a small movable laboratory might be constructed at very little cost to obviate all difficulties ; and by the summer of 1888 a convenient portable hut was eventually obtained. Thus was inaugurated on a small scale the systematic study of the fauna and flora of the European lakes, which has since been carried on more exhaustively elsewhere, and which led to the establishment of the well-known station on the Lake of Plön (Holstein), opened by Dr. Zacharias in 1892.

The Dutch had already constructed a small portable station for the study of the life on the coast of Holland, and Dr. Fritsch arranged his building on much the same plan. A view of the exterior is given in Fig. 1, and of the interior in Fig. 2. The structure is of wood, consists of about 80 parts, and has a total weight of 1,000 kilogrammes. It can be erected in two hours and a half, pulled down in one hour and a half. The floor space is 12 square metres, and on

[1] Untersuchungen über die Fauna der Gewässer Böhmens.—IV. Die Thierwelt des Unterpocernitzer und Gatterschlager Teiches, als Resultat der Arbeiten an der übertragbaren Zoologischen Station. By Professor Dr. Anton Fritsch and Dr. V. Vavra. *Archiv. d. naturw. Landesdurchforschung von Böhmen*, vol. ix., no. 2, 1893 (1894). [This work is well illustrated, and we are indebted to the authors for the loan of the four photographs here reproduced.]

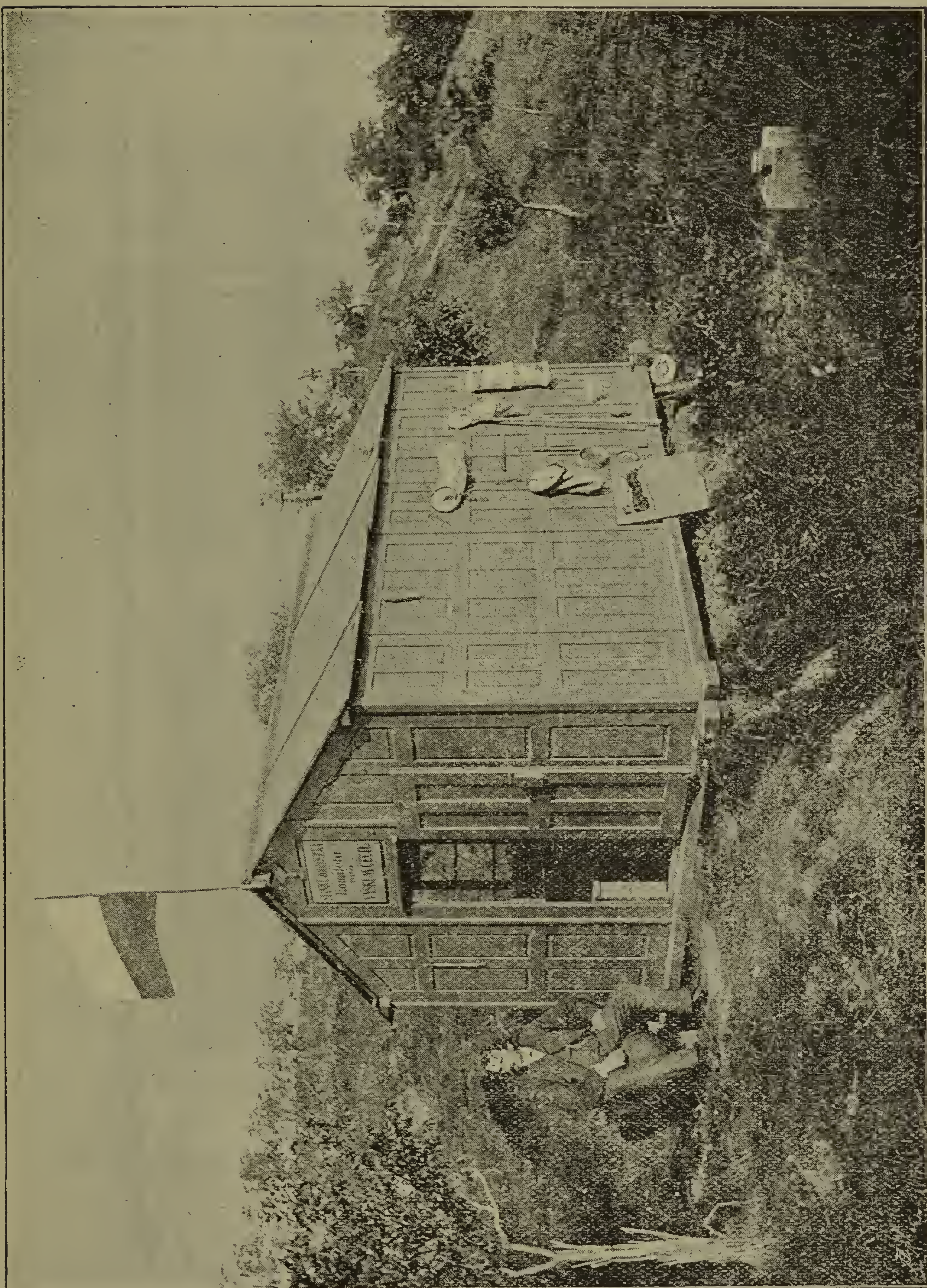


FIG. 1.—The portable Zoological Station of the Commission for investigating the Natural History of Bohemia.

the northern side there are two windows, of which the shutters fall inwards and form two large work-tables (Fig. 2). At night, or whenever required, these shutters can be turned upwards and the apartment readily converted into a sleeping room. The walls are conveniently fitted with shelves and hooks, and there is a small stove for warming.

The fishing apparatus is shown in the third photograph (Fig. 3). In this figure, no. 1 represents the net for surface-captures, made of thick silk, with two rings of 20 cm. diameter (one brass, the other cane), and one ring of 10 cm. diameter fixed to the neck of the bottle. No. 2 is a net on a bamboo rod for shore fishing and for depths of

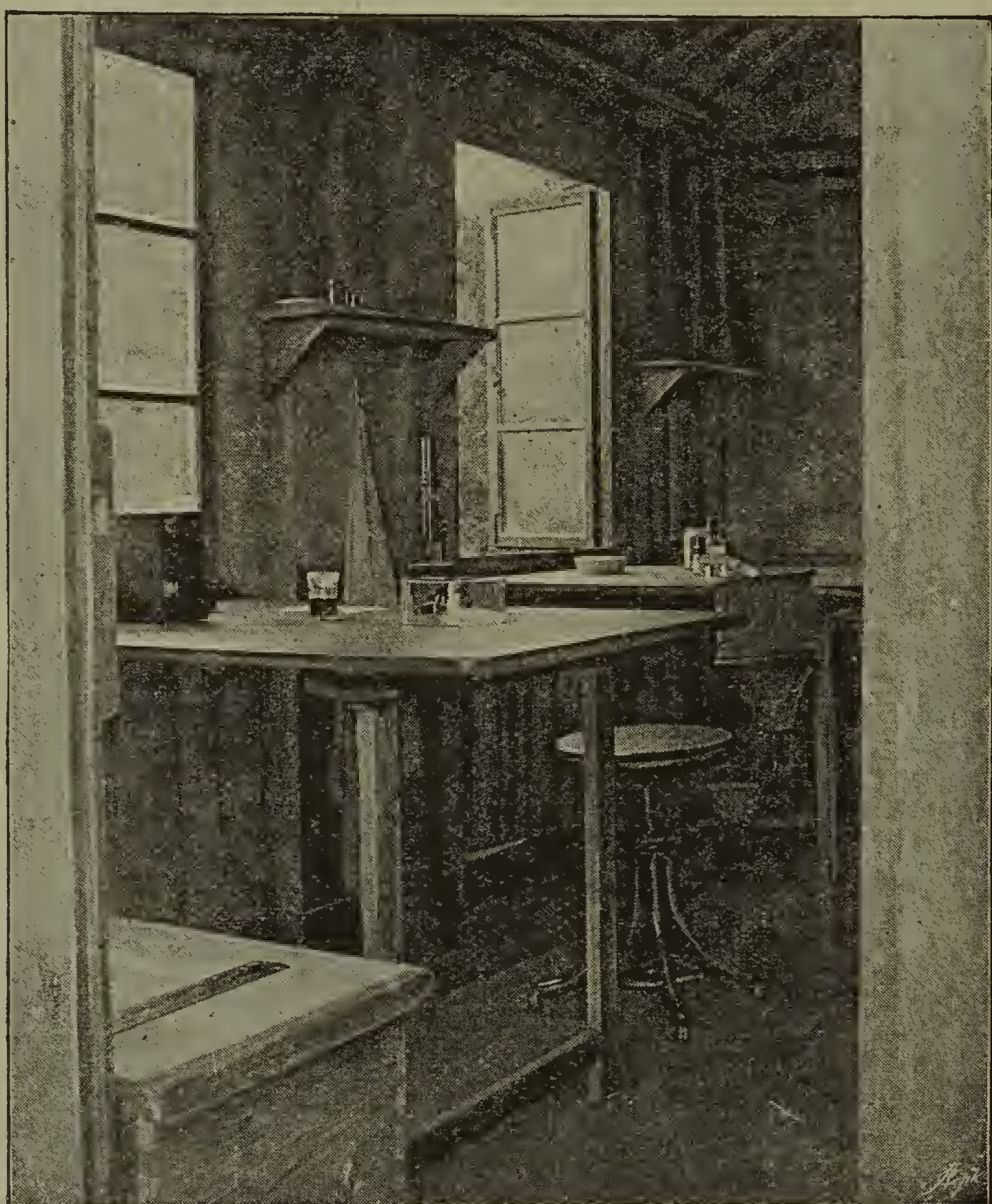


Fig. 2.—View of the Interior of the Bohemian portable Zoological Station.

one metre. No. 3 represents a deep net with a simple arrangement for closing at any given moment. It consists of two parts 30 cm. in length, with an inner conical part leading to the bottle. No. 4 is a net for dredging mud. Nos. 5 to 8 are respectively a measure, thermometer, and two metal sieves for washing mud. No. 9 is a weight used in measuring depths, and no. 10 is a holder for glasses. Nos. 11 to 13 will be recognised as an aquarium and water-cans, while no. 14 is a plan of a lake. A common small hand-net is also used, and there is a light boat for the service of the station, but these are not shown in the illustrations.

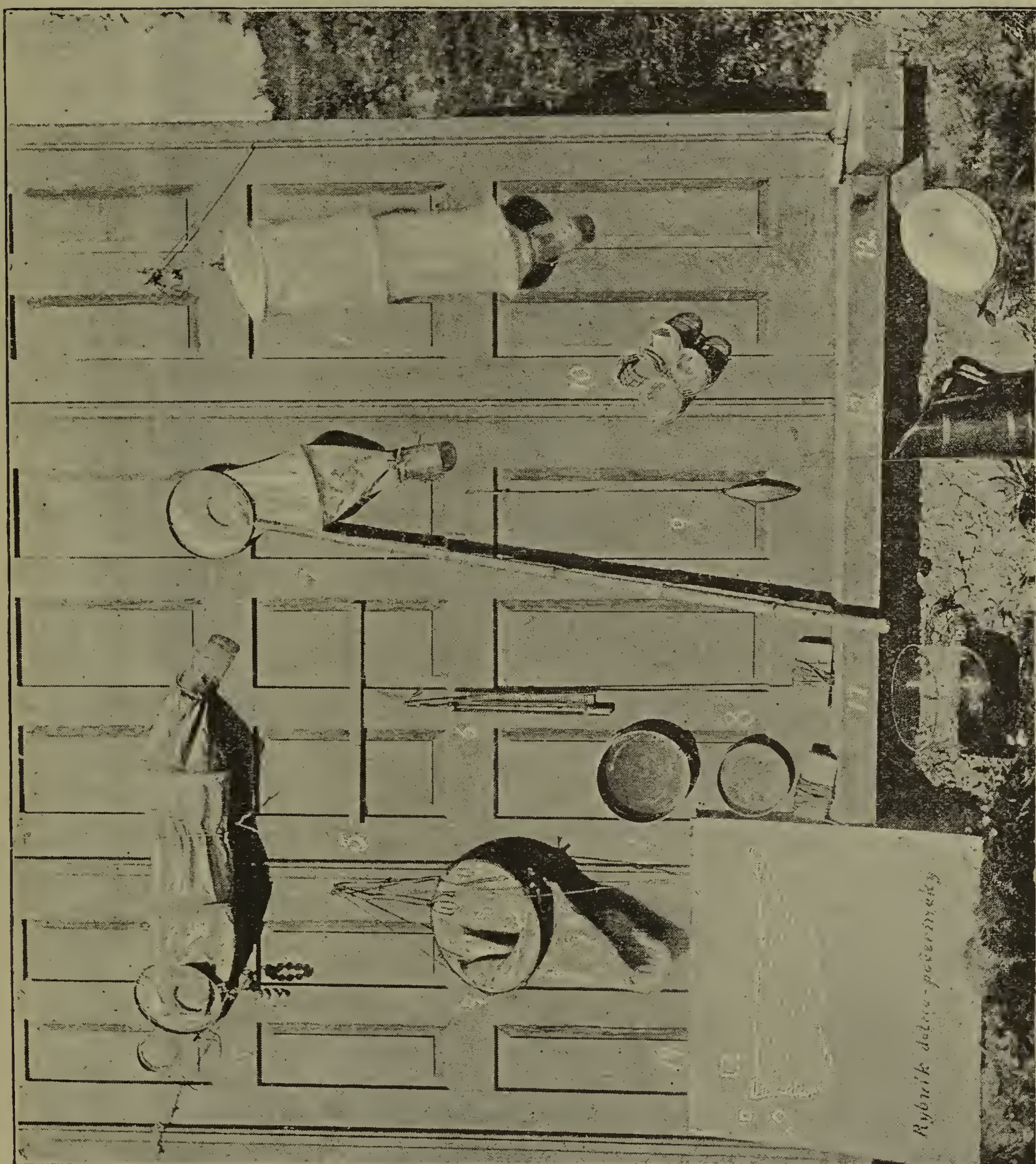


Fig. 3.—Apparatus for investigating the Freshwater Fauna of Bohemia.

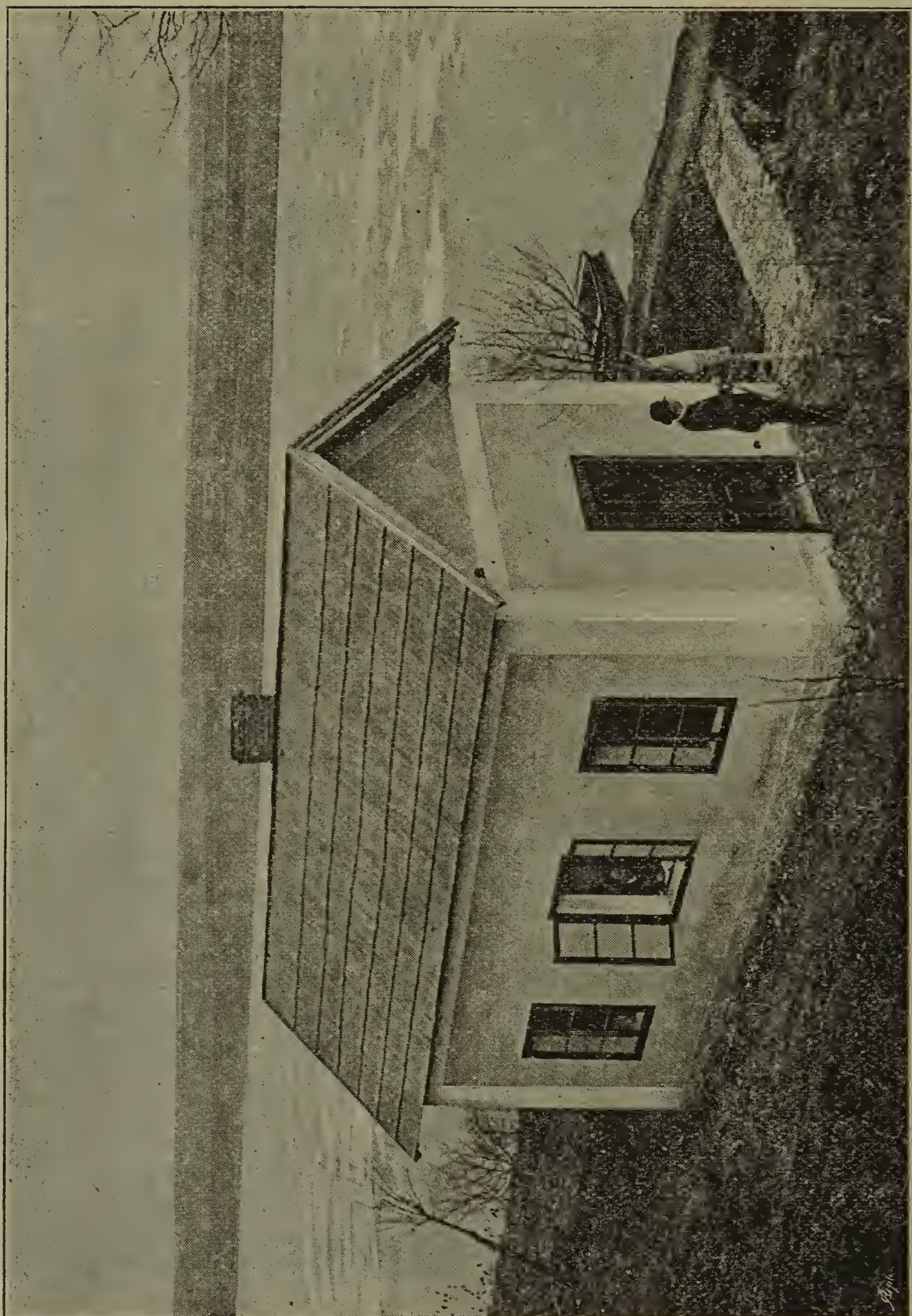


FIG. 4.—The permanent Zoological Station established by Baron Bela Dercsenyi on Lake Unterpocernitz, Bohemia.

Thus equipped, Dr. Fritsch's portable Zoological Station has done good service; and when it appeared necessary to remove it to the south of Bohemia in 1892, the incomplete researches on the Unterpocernitz Lake were regarded as so promising that the local landowner, Baron Bela Dercsenyi, was induced to erect a permanent station on its shore on a somewhat enlarged scale. A photograph of the building due to the baron's generosity is reproduced in Fig. 4. It was handed over to the Bohemian Commission in July, 1892, and a large part of the report on which this article is based is occupied with the results of work accomplished here. In the permanent station there is a small second chamber which can be used for meals and sleeping accommodation; but the laboratory itself is arranged much as in the portable hut already described.

Progress in researches of the nature undertaken in these stations is necessarily slow, and the time has scarcely arrived to utilise the results in general deductions. Drs. Fritsch and Vavra, in the report now before us, are content to present a well-illustrated synopsis of the fauna and flora of the Unterpocernitz and Gatterschlag Lakes, merely alluding to the wider problems which remain to be solved. In the systematic list there are notes on the varieties of each species, its time of appearance, and normal circumstances of growth; and many of the illustrations are original. We congratulate the naturalists of Bohemia on their enterprise, and heartily wish them the success they deserve. We only regret that we cannot yet chronicle any similar work in our own more highly-favoured land. But one can hardly hope for any extension of our energies to freshwater, while the study of marine biology remains in the unpromising state to which we allude in our "Notes and Comments" for this month.

SOME NEW BOOKS.

The Agency of External Influences in Development.

Aeussere Einfluesse als Entwicklungsreize. Von August Weismann, Professor in Freiburg i. Br. Jena: Verlag von Gustav Fischer, 1894.

The Effect of External Influences on Development. The Romanes Lecture, 1894. By August Weismann, M.D., Ph.D., D.C.L., Professor in the University of Freiburg in Breisgau, with annotations by the Author. London: Henry Frowde, Amen Corner, E.C. Oxford: Clarendon Press Depository, 1894. Price 2s.

We have before us Dr. Weismann's lecture, published by the Clarendon Press, and purchased by ourselves, and the German edition sent us with the usual courtesy of the German publisher. The preface in the two editions is slightly different. Both contain a kindly and appreciative reference to the late Professor Romanes. The German preface calls attention to an additional note added at the end of the German text, and dealing with some of the matter contained in Hertwig's "Preformation or Epigenesis," an account of which has recently been published in Natural Science.

The German text is the original from which the translation was made by Mr. Gregg Wilson. It certainly serves to show that Professor Weismann is not so oblivious of the agency of external influences in development as Oscar Hertwig implies in his recent criticism. It deals, not with the whole field suggested by the title, but specially with the agency of certain external influences that call into activity varied constituent parts in the process of development. It is, then, in no measure a withdrawal from the preformationist position. It is an argument to show that many external influences that have been supposed to affect the process of development as direct causes in the epigenetic sense, that in fact have been supposed directly to stamp certain characters upon organisms, stamp these characters not directly but by calling into activity latent but existing constituents of the germ-plasm. As was explained in his book, Weismann believes that in the germ-plasm that gives rise to the organism there are present many sets of parts, each of them capable of producing the whole organism. In the actual development, when the germ-plasm is disintegrated into the separate parts or determinants which control the production of the separate variable parts of the animal, for each separate independently variable part there are many determinants present, and hence a struggle ensues between these as to which of them shall acquire the mastery and guide the growth of the organ in question. When the environment seems to have an important direct effect, Weismann believes that it is, so to speak, merely interposing in the struggle between the determinants, egging on some of them that otherwise

would have been vanquished or might have remained dormant, and preventing others from acquiring the control that otherwise would have fallen to their lot.

He expounds and enlarges Roux's theory of intra-selection. If an individual development were predetermined in every detail, it could no more produce an organism fit for life than, as Roux has aptly put it, "would a commander be victorious, who, instead of giving general instructions to his chief officers as to the placing and movement of their troops, should in advance issue detailed orders for the conduct of everyone down to the lieutenants, or even to each private soldier. The influences which encounter organisms during their development are never exactly similar, and to adapt themselves to these, the organisms must have a certain amount of freedom." The necessary freedom is given by intra-selection, for that "effects the special adaptation of the tissues to special conditions of development in each individual."

Taking the special case of colonial insects, in which there are males, females, and neuters, he elaborates an argument to show that the direct effects of poor nutrition are not sufficient to account for the differences between neuters and females, but that "the poor nourishment acts as the stimulus for the latent primary constituents for the workers in the germ-plasm, not only for those in the ovary, but also for those of all characters by which the worker is distinguished from the queen."

The origin of these materials on which intra-selection and the stimulus of external agencies act, Weismann refers to variations in the germ-plasm.

The Development of the Idea of Evolution.

From the Greeks to Darwin: An Outline of the Development of the Evolution Idea. By Henry Fairfield Osborn, Sc.D. 8vo. Pp. 260. New York and London: Macmillan & Co., 1894. Price 9s. nett.

The Columbia University of New York has decided to issue a series of small volumes on Biology, under the editorship of Dr. Osborn, the Da Costa Professor of Biology. It is not to be a library of elementary text-books in the ordinary sense of the term, but to consist chiefly of works dealing with certain fundamental problems which will concern students who have passed beyond preliminaries. With this aim it is therefore appropriate that the first volume of the series should relate to the history of the philosophy of the subject. We are glad now to welcome such a work from the pen of Professor Osborn, who has industriously compiled a most interesting and useful treatise. It is based partly upon a course of lectures delivered at Princeton in 1890, partly upon another course delivered last year in New York.

Professor Osborn's treatment of the subject, though largely following in the lines of previous authors, differs from preceding attempts in exhibiting more continuity. He shows more clearly than has hitherto been done that the growth of the idea of evolution has been continuous from the efforts of the earliest Greeks to the views of the present day. He carefully follows the broad idea of Evolution as a natural law, and traces back the birth and development of each of its parts, while constantly keeping in mind the changing environment of knowledge and prejudice. As a treatise for the advanced student before entering upon special reading and

research, it would indeed be difficult to imagine one more fairly balanced; and the Professor has been singularly successful in emphasising the salient points.

Evolution, as a natural explanation of the origin of the higher forms of life, succeeded the old mythology in Greece, and first developed from the teachings of Thales and Anaximander (B.C. 611–547) into those of Aristotle. This great philosopher had a general conception of the origin of higher species by descent from lower species, and he even stated the theory of the Survival of the Fittest, though rejecting it as an explanation of the evolution of adaptive structures. He also believed that there was no fortuity in Evolution, but that the succession of forms of life was due to the action of an internal perfecting principle originally implanted by the Divine Intelligence. In short, the ancient Greeks, with Aristotle as their most brilliant genius, had already discussed the problem of life from three points of view — had enquired whether Intelligent Design is constantly operating in Nature, whether Nature is under the operation of natural causes originally implanted by this mysterious Design, or whether it is governed by natural causes exclusively due to the laws of chance from the very beginning.

During the long Middle Ages the theory of Evolution made no advance, and even in the end retrogressed. Subsequently the rigid conceptions and definitions of species developed in the rapid rise of systematic Zoology and Botany were grafted upon the Mosaic account of the Creation, establishing a Special Creation theory for the origin of each species. Later still, when it was discovered from Palæontology that species of different kinds had succeeded each other in time, the "Special Creation" theory was again remodelled to cover a succession of creations extending down almost to the present day. Thus an ecclesiastical dogma developed into a pseudo-scientific theory full of inconsistencies, but stoutly maintained at the time by leading zoologists and botanists.

In the seventeenth and eighteenth centuries three classes of writers contributed more or less directly to the foundation of the modern study of Evolution. First, there were the industrious naturalists collecting facts; secondly, the speculators who still held many of the false Greek notions; and thirdly, the great Natural Philosophers, of whom Bacon, Descartes, and Leibnitz are examples. Of these it is curious to note that the third class did most towards facilitating modern progress. They clearly perceived that the point to which observations should be directed was not the past but the present mutability of species, and further, that this mutability was simply the variation of individuals on an extended scale. In short, these philosophers recognised the gradations of type, the facts of variation, and the bearing of these facts on the production of new species; they even noted the analogy between the artificial selection practised by man in producing new forms of plants and domestic animals and the production of new forms in Nature. At the same time, it must be added that the Evolution of Life was only made a very subsidiary element in the System of Philosophy they established.

Coming to later times, Professor Osborn deals very fully and in general fairness with the respective claims of modern evolutionists. His opinion of Treviranus is less high than that commonly held, and he attempts to assign a very definite position to Erasmus Darwin. The concluding section of the book, however, contains more familiar matter than the preceding chapters, and it is unnecessary here to

reproduce an outline of the history. We have merely tried to give a general idea of the scope of the volume, which will be welcomed by all who have not the time or opportunity to refer to the original works on which it is based. We need only add that those, too, who desire to study the subject more deeply will find the bibliography at the end a useful guide.

The Great Ice Age.

The Great Ice Age, and its Relation to the Antiquity of Man. By James Geikie, LL.D., D.C.L., F.R.S., etc. Third edition. Largely rewritten. London: Edward Stanford, 1894. Price 25s.

This is a book which, in my view, ought not to have been written. When Mr. James Geikie, in 1873, published the first edition of his "Great Ice Age," it was welcomed by every geologist as a laborious, ingenious, and able argument in favour of a position which was widely held. It contained abundant proofs that its conclusions were based on a wide induction, and whether they were right or wrong, whether they were ephemeral or likely to live, it made no difference to sensible men, who know that most theories are tentative and that it is always a supreme advantage to have even an extravagant theory worked out by an able man.

When, after twenty years, Mr. Geikie brings out a new edition of his work, largely rewritten, we expect from him, not merely in his capacity as an experienced geologist, but as the occupant of an influential chair in the University of Edinburgh, and responsible for teaching many young men, that he shall have carefully read and discussed the various objections which have been urged against his facts and his conclusions, and that he shall either answer them or frankly say that fresh light has modified old views. Such a work would have been of great advantage to us all and would have enhanced Mr. Geikie's reputation where, I suppose, he chiefly wishes it to be enhanced, namely, among the younger geologists whose day of triumph is not yesterday or to-day, but to-morrow.

Instead of this, what have we? In the first place, a parade of authorities which is really most misleading. Names there are in abundance, some with reputations and some without, but they are virtually all on one side. Of the men quoted and the men whose views are alone considered, some were long ago committed to the same views, and others belong to an official school having an official creed to maintain. What use, therefore, is such a book to anybody? Who is likely to buy it, or read it, or to learn anything from it, except the unwary amateur, who will not realise that almost every position maintained by Mr. Geikie has been controverted, and that a large part of his arguments are nearly as obsolete as the geological science of De Saussure and De Luc?

Mr. Geikie declines to traverse the unwelcome literature where the history of the glacial theory is written. The consequence is that very many so-called discoveries chronicled by Mr. Geikie were made by men long ago, whose work ought to have been recognised.

There is also a great deal of modification of old views, and of climbing down. How much, only those will realise who take the pains to carefully compare the two editions; but instead of this being frankly and openly confessed, it is disguised under a specious form of rhetoric.

Thus Mr. Croll, who was the *Deus ex machinâ* of the first two editions, the inspired prophet who had solved everything, and against whom it was almost treason to argue, is treated very differently in

the present one. The chapter on the molecular theory of glacial motion is cancelled. This is peculiarly gratifying to me. For years I have been trying to champion Forbes' theory of glacial motion, and have written a great deal on the subject. I have always deemed it to be the pivot of any possible glacial theory. When Mr. Geikie brought out his previous edition he brushed Forbes' theory aside as impossible, and devoted many pages to a theory invented by Croll, which has been the amazement and amusement of many of us, namely, the molecular theory of ice motion. This is now thrown over without a word. Forbes' theory is completely accepted, and one of the old masters justified. So far, so good. And yet Croll was right. He wanted ice to do some wonderful things which a viscous body will not do. Forbes' ice—the ice we all know and can experiment upon—was no use to him. He had, therefore, to invent a special ice of his own. His scholar, Mr. Geikie, wants to do Croll's work with Forbes' tools. Assuredly, to change my metaphor, the old wine cannot exist long in the new bottles. Let us proceed, however. The chapters on cosmical changes of climate are now condensed and remitted to a short concluding chapter, with an ominous change in its heading—it is now entitled "Cause of the Glacial Period"—and to foot-notes full of ambiguous surrender. We are even told that if Mr. Croll's views happen to turn out to be wrong, still Mr. Geikie's geological conclusions will remain. All this is very curious—curious rather as a study of human nature than of science.

Let me next refer to one or two critical instances of Mr. Geikie's method of ignoring his opponents and critics. He devotes two considerable chapters to the vexed question of the origin of rock basins and mountain lakes. In this he repeats the kind of arguments which Ramsay long ago published, and he largely quotes from Mr. Wallace, who is a distinguished zoologist, but whose reputation in geology has still to be made. Will it be believed that not a word of any kind is said about the arguments on the other side, urged by the Duke of Argyll and Mr. Bonney, both ex-presidents of the Geological Society of London, nor of those of Mr. Spencer, who is in the very first rank of American geologists, which arguments have simply pulverised the whole theory of glacier erosion of such lakes? It is literally one of the most impertinent incidents in all my experience of literature, that Mr. Bonney's elaborate memoirs on this subject, directly aimed at Mr. Wallace and Mr. Geikie himself, should be completely ignored—Mr. Bonney, who has probably spent as many months in exploring the handiwork of ice in Switzerland and Norway as Mr. Geikie has spent days, and who has inductively analysed this particular problem with great sagacity and experience on the spot, where the lessons can be really learnt, and not by a process of deduction from impossible mechanical data.

In regard to the astronomical theories of an ice age as urged by Croll and Sir Robert Ball, we are treated to a *réchauffé* of old arguments which have been riddled through and through, not merely by geologists, but by mathematicians and astronomers, like Mr. G. Darwin, Mr. Meach, in America, and, lastly, by Mr. Culvewell, who at the British Association at Oxford assuredly gave a final *quietus* to the whole subject in an analysis of its conditions, whose force was acknowledged by Sir R. Ball himself. Not a word is said of all this, but the ingenuous youth of Scotland is led to believe that the position is still intact, and that there is still something to be said for astronomical cycles causing climatic revolutions in the earth.

On the question of antarctic ice, Mr. Geikie, following Croll and ignoring all that has been written by Lord Kelvin and others, actually postulates a depth of 12 miles of ice as a probability. As if ice under such a pressure would not crush and liquefy and flow away, and as if the accumulated evidence of all the explorers available did not go to show that the antarctic ice has, in fact, a very moderate depth.

Next, take the question of Greenland and its contour, upon which a good deal depends. We are again treated to the story of Greenland being a mass of islands, most of them low, which are supposed to be covered with a stupendous mound of snow and ice. All the work of the Danish exploring expeditions in mapping out the Nunatakker, and going to show that the great island is in reality a high plateau, is ignored in favour of Mr. Geikie's old view. This is now supported by an *obiter dictum* of Mr. Nansen, who is not a geologist, and whose *dictum* is as conclusive as that of any other traveller who infers the depth of a deposit from the appearance of its surface. As a matter of fact, it is as easy to know what depth a snow mass is from walking over it as it is to know the depth of the ocean from sailing over it, or the contents of a pie from an inspection of its crust. Nor, again, in what is said about the glaciation of Greenland, is any reference made to the proofs recently afforded by Danish botanists that the flora of Greenland, instead of pointing to its recent emergence from much more glaciated conditions, points exactly the other way.

Again, take the question of a ground-moraine, the invention of an ingenious Frenchman, who had probably never seen a glacier, and the main buttress of many geologists who have never seen glaciers either. This particular postulate is used with the greatest confidence and assurance, as if it had ever been proved or even admitted. Mr. Bonney has, in this instance again, shown what a fantastic postulate it is as usually quoted. But it does not need Mr. Bonney's assurance. Anyone who has carefully examined glaciers at work in Switzerland and Norway can judge for himself as to this monstrous invention. Where is anything like the supposed ground-moraine being formed by ice now? How is it possible to explain its formation by any mechanical theory? Of course not. *Glacial streams* sometimes form a layer of rounded boulders, and sometimes, very locally, small beds of stratified sand; but how can we suppose that ice, whose qualities are pretty well-known, can, under the enormous pressure of 5,000 or 6,000 feet, actually move under its foot masses of 500 or 600 feet deep of soft beds (in many cases of sorted beds)? The same ice which, when it has moved scores of miles from its original home, is supposed to have such erosive power that it can scoop out great lakes, and such momentum that it can travel up-hill for hundreds of yards! yet this ice is not only to thus carry *under its foot* these beds, but to do so without breaking the shells sometimes present in it, and without kneading the assorted beds into a heterogeneous mass of boulders, clay, and sand. Assuredly the ice referred to is Saturnian ice and not Mundane. As assuredly the head of the Geological Survey of Scotland ought either to substantiate his postulate by some rather more reasonable arguments or leave it to the followers of Swift and of Rabelais.

Again, as to subglacial water. All kinds of things are postulated of it. For instance, while water running in rivers runs according to gravity, and follows valleys and watercourses in a rational way, subglacial waters are supposed to have been quite indifferent to the contour of the country, and to have gaily travelled over hill and dale,

sorting the clays from the sands, and arranging the eskers and drumlins in places where it is impossible to understand how any water could flow at all, and to have flowed, not in the form of streams but sheets of water. The position would be laughed out of court at once if applied to subaërial streams and rivers, but when they are buried under ice anything seems possible.

Lastly, the old Norwegian ice-sheet is trotted out, and nothing said of the arguments and facts collected by Pettersen and Kjerulf in Norway, and by Mattieu Williams and Bonney and others in England, showing that the whole thing is a dream.

I have merely quoted a few samples of the kind of geology which may be found in this book. They are only samples. Every chapter, and almost every paragraph, contains similar præ-Baconian logic, and if I had room I might go on for ever.

One word in conclusion, which is personal and necessary. It would, in fact, be impossible for me to sign this article without mentioning it. In referring to the works ignored by Mr. Geikie, I have preferred to refer to those of better men than myself; but I am bound to say that I cannot quite ignore myself. I have written two fat volumes and a great many papers on the subject chiefly in the *Geological Magazine*, and others, in *Nature*, both of them publications of high reputation. In them I have examined, with courtesy and with such pains as I was capable of using, nearly every position maintained by Mr. Geikie, and have had the advantage of the support of most responsible critics who have at great length reviewed my books in the best reviews, and of many distinguished private critics both physicists and geologists. I had a right, therefore, to expect that some attempt would have been made to meet my objections. It is no use in these days supposing that the democratic wheels of Science are going to spare the self-sufficient, and the self-engrossed, and the self-opinionated. Every man whose work is to live must face his critics with frankness and with pains, if the criticism is courteous and rational; and those who will not do so, but go on repeating old wives' tales that have been exploded long ago, will find that the tide has mercilessly swept away the sand-heaps they have built. With these unsubstantial buildings may also go, if we have no care, reputations that are based on something better than the truth of certain theories, namely, long and devoted labours like Mr. Geikie's in the field.

Henry H. Howorth.

A Rare Book on American Zoology.

A Reprint of the North American Zoology. By George Ord. Being an exact reproduction of the part originally compiled by Mr. Ord for Johnson and Warner, and first published by them in their Second American Edition of Guthrie's "Geography," in 1815. Taken from Mr. Ord's private, annotated copy. To which is added an appendix on the more important scientific and historic questions involved. By Samuel H. Rhoads. Published by the Editor, Haddonfield, New Jersey, 1890. Pp. x., title page, pp. 290-361, and appendix pp. 1–52, and "index to whole volume," pp. 53–90. With a portrait of Ord. Price $3.

It is perhaps hardly an exaggeration to say that no person on this side of the Atlantic has ever seen Ord's "Zoology." The book is as well-known by name among mammalogists as Linnæus's "Systema Naturæ," on account of the endless controversies relative to the nomenclature of the grizzly bear. In all discussions on the relation-

ships and nomenclature of that animal, Ord's "Zoology" has been quoted. Baird cited a copy of the work in 1857 as "exceedingly rare," and Coues refers to an imperfect volume as "the only copy I ever handled." The history of the "Second American Edition" of Guthrie's "Geography," in which publication Ord's memoir appeared, is involved in obscurity. Both Baird and Coues used the copy in the possession of the Philadelphia Academy of Sciences, but that copy has mysteriously disappeared. The only other recorded copy has been unearthed by the diligence of Mr. Rhoads, who learned that Ord had presented a quantity of books and papers to the Philadelphia College of Physicians. On enquiring at the College, it was not found in the library, but information was forthcoming that Dr. J. Solis Cohen had purchased from the College a number of books that were not considered to come within the scope of the library. Dr. Cohen made a search among his books, and, to the joy of the enquirer, found not only a perfect copy of the work, but Ord's own copy containing marginal pencil notes on the zoological portion. Zoologists owe, therefore, a debt of gratitude to Mr. Rhoads for his painstaking search, and to Dr. Cohen for permitting the reprinting of so rare and interesting a treatise.

Owing to the worn and imperfect condition of the type, a photographic reprint was impracticable, but judging from the evident care and pains taken by Mr. Rhoads we feel no hesitation in accepting this as a faithful reprint of the "Zoology." Ord's name does not appear, and the editor of the "Grammar" says, "the modesty of its author forbids a personal acknowledgment, which the editor would have the highest satisfaction in making." But that Ord was the author there is absolutely no doubt, and to him belongs the credit of priority of nomenclature for numerous American animals.

The "Zoology" commences on p. 290 of the "Grammar," with a brief note to say that the author has arranged the orders and genera according to Turton's edition of Linnæus. Pp. 291, 292 are occupied with a list* of the animals, of which both the common and binomial designations are given, and it is on p. 291 that the name *Ursus horribilis* (Grizzly Bear) occurs. Pp. 293–313 are devoted to notes and observations on the mammalia given in the list, but no scientific descriptions are attempted. The list* of American birds occupies pp. 315–319, and the next thirty-six pages are devoted to notes and observations similar to those given on the mammals. A list* of amphibia occurs on p. 357, and after a few notes on these, the fish and remaining orders of animals are dismissed in about a page of text.

In the appendix which Mr. Rhoads has added to the work, he has been assisted by Dr. Coues, Mr. Witmer Stone, and Professor Cope, and the result appears to be an excellent and careful digest of the nomenclature of the animals quoted by Ord and a valuable addition to American zoological literature.

The new names proposed by Ord are here given; those printed in italics apparently hold good:—Vespertilio americanus; V. rubra; Viverra albus; Mustela hudsonius; *Ursus horribilis* (stands if specifically distinct); Mus *tuza*; M. *cinereus*; M. canadensis; M. *pennsylvanica*; Arctomys *ludoviciana*; A. *columbianus*; Sciurus *pennsylvanica*; S. hiemalis; S. carolinensis; S. labradorius; Antilope *americanus*; Ovis montanus; Vultur columbianus; Falco cæsius; Picus

* These lists (the only part of scientific value) did not occur in any other editions.

montanus; Phasianus *columbianus*; Tetrao *fusca*; Sterna *philadelphia*; *Larus delawarensis*; Anas *columbianus*. The reptiles given in "Amphibia" under new names are, Professor Cope says, "either synonyms or unidentifiable." Mr. Rhoads has published in the *American Naturalist*, June, 1894, pp. 523–526, a summary of changes in nomenclature proposed, and to this the critical reader is referred.

A few errata are carefully inserted on p. 52, and an index of over 4,000 entries enables ready reference to any names used either in Ord's work or Rhoads' appendix. C. D. S.

Deep-Sea Deposits.

Carte des Sédiments de Mer Profonde, avec notice explicative. By John Murray and A.-F. Renard. 8vo. Pp. 45 and map 43 by 25 cm. Bruxelles: Société Belge de Librairie (Société Anonyme), Oscar Schepens, Directeur, 16 rue Treurenberg, 1894. Price 2 fr. 50.

Those who are fortunate enough to possess the first number of Natural Science will find therein a long review of the "Challenger" Report on Deep-Sea Deposits. The pamphlet now before us is practically an epitome of the contents of that volume by its authors, and is published so as to bring within reach of all the main facts and results detailed in the Report.

The text appended to the present map, which appeared originally in the *Bulletin de la Société belge de géologie*, vol. viii., 1893, consists of a concise summary of the various types of sediment distinguished by the authors, exclusive of the micrographical and chemical details and theoretical considerations provided in the bulkier Report. It presents all the facts on which are based the construction of a submarine geological and mineralogical map.

The descriptions commence with the nature of the deposit (red-clay, blue-mud, globigerina ooze, radiolarian ooze, etc.), and follow with a general determination of the macroscopic characters of the deposit. The sediments were treated with hydrochloric acid, and both the soluble and organic contents are given. The residues, after washing, were sorted into three groups, which are given under the headings of Siliceous organisms, Minerals (particular minerals and rock fragments), and Amorphous matter.

The pamphlet concludes with a chapter on the geographical and bathymetrical distribution of deep-sea deposits. The map, which is clearly coloured, shows at a glance the depths of the oceanic areas in *brasses* (100 brasses = 183 mètres), the lines of depth being drawn at 100, 500, 1,000, 2,000, 3,000, 4,000, and deeper than 4,000 brasses; and the deposits are coloured and defined as muds and coral sand, globigerina ooze, diatomaceous ooze, radiolarian ooze, pteropod ooze, red clay, and blue muds.

The information about deep-sea deposits thus presented in so convenient a form cannot fail to be of the greatest service.

The Fisheries of the Mediterranean.

Les Pêcheries et les Poissons de la Méditerranée (Provence). By Paul Gourret. 16mo. Pp. 360 with 109 figures. Paris: J. B. Baillière & Sons, 1894. Price 4 fr.

This little work, by the sub-director of the Zoological Station at Marseilles, forms a volume of Messrs. Baillière's Library of Useful Knowledge. It is an interesting treatise on the fishes and fisheries of

the Mediterranean, well written and profusely illustrated, and likely to be of value, not only to the specialist, but also to the general reader desiring some authentic information on the subject of which it treats. The first part deals with the various fishing stations, giving an account of the configuration of the coast, the nature of the sea-bottom, the depth of the water, and the winds and currents. The second part relates exclusively to appliances, is especially well illustrated, and occupies by far the greater portion of the book. The third and fourth parts explain the influence of surrounding conditions on the distribution and varying abundance of the different fishes of economic value, and detail the precautions necessary for the successful protection of the fishing industry in Southern France. The final chapter gives an admirable list of the fish-fauna, including the scientific and popular names of each species, together with its habitat, abundance or rarity, and modes of capture.

The Flora of Nebraska.

Flora of Nebraska. Edited by the Members of the Botanical Seminar of the University. Introduction; and Parts 1 and 2. Pp. 128, plates 36. Published by the Seminar, Lincoln, Nebraska, U.S.A. Price $1 per part.

The botanical Seminar of the University has undertaken the publication of a complete "Flora of Nebraska," to be issued in twenty-five parts by subscription at the rate of one dollar per part. Each part will be the work of a specialist, and will be in the nature of a monograph of the group under consideration. The parts first to appear will deal with groups of which the literature is so diffuse as to be inaccessible to all save those who have constant access to the botanical library of the University. These will also be the more profusely illustrated. This is a great point in the new publication, and results in the disposal of three-fifths of the whole work on the cellular plants, representing fifteen parts, while nine are devoted to the few vascular cryptogams and the flowering plants. Number twenty-five will contain a catalogue or check-list, and also a list of host-plants of fungi. The aim of the Seminar is most commendable, and we wish them success in their project; the "lower plants" generally receive but poor treatment in a flora, and their study in consequence is neglected. Parts 1 and 2, which have been issued together, augur well for the usefulness of the work; part 1 (Protophyta to Phycophyta) is by Alton Saunders; part 2 (Coleochætaceæ to Characeæ) by Albert F. Woods. Professor Bessey supplies an introduction to the whole. *A propos* of the introduction, the plan is adopted of replacing the familiar term *order* by *family*, while the former represents the *tribe* of Hooker and Bentham. For instance, the polypetalous dicotyledons are subdivided into the orders Thalamifloræ, Discifloræ, and Calycifloræ, the old cohorts becoming suborders, while the old natural orders are families. It would be a great advantage if systematists could agree on the terms to be applied to the various subdivisions; the want of a uniform code is very confusing to the student, who can never be quite clear what is meant to be conveyed by the chance mention of a given term.

The Geological Survey of the United Kingdom.[1]

The Report of the Director-General of the Geological Survey has recently been issued for the year ending December 31, 1893. This

[1] See Report of the Science and Art Department for 1893, published by H.M. Stationery Office.

shows an excellent record of work done, and due credit is given to the admirable weather which permitted a survey of 200 square miles in excess of that surveyed in the previous year.

Nine new sheets of the one-inch map of England and Wales were issued during the year in two editions (Solid and Drift), while forty-nine MS. coloured copies of the six-inch maps have been completed and deposited in the office for public reference.

In the record of field work we find that Mr. Fox-Strangways has revised the mapping of the Charnwood Forest area, and determined the overlap of the New Red Marl. The contact of the eruptive rocks with the altered strata has also been observed at Badden Wood and Garenden, while much detail has been ascertained of the topography of the land that sank beneath the Keuper Sea. Mr. Lamplugh has been engaged in studying the "Skiddaw Slates" of the Isle of Man, but has as yet not obtained any clear idea of the succession. He has obtained a few obscure organisms from the slates, but nothing of any palæontological value. The red sandstones to the north of Peel have, however, yielded more tangible evidence, and the corals found, though specifically indeterminable, seem to point to Upper Silurian or Devonian age.

In the Devonian area, the presence of *Entomides* near Tor Point establishes the presence of Upper Devonian rocks to the west of Plymouth Sound, while the Plymouth district has been brought into connection with those of Newton Abbott and Torquay.

Messrs. Strahan and Gibson have completed sheet 249 of the one-inch survey of the South Wales Coal-field, and sheets 232 and 263 are in progress. By tracing the outcrops of certain coal seams, Mr. Strahan has ascertained approximately what part of the Pennant Rock forms the surface of the ground of the anticlinal ridge which separates the Caerphilly and Llancaiach basins, information of considerable commercial importance.

The Permian and Trias areas are mainly under the observation of Mr. De Rance, who, among other matters, has made a detailed examination of the sections exposed in cutting the Manchester Ship Canal.

Some few new localities for Corallian fossils in the great Oxford Clay series of Huntingdonshire and Bedfordshire have been found by Mr. Cameron; while Mr. H. B. Woodward and Mr. Strahan have examined and photographed the Jurassic beds exposed in the Dorsetshire cliffs between Swanage and Weymouth for the purpose of preparing a detailed section.

The subdivisions of the Chalk have been studied by Messrs. Hawkins and Jukes-Browne, and much interesting information has been obtained on these points. The Chalk Rock of Dorset varies much lithologically, and at Winterbourne Abbas two beds of hard brecciated rock were found to be separated by five or six feet of softer glauconitic chalk containing large grains of quartz. The brecciated material contains small fragments of fine-grained greensand, which, as Barrois has noted, correspond with varieties of Upper Greensand, indicating possibly that the Upper Greensand was undergoing erosion not far from this place during the formation of the Chalk Rock.

In the Tertiary deposits the true position of the Ramsdell Clay has been cleared up, and Mr. Blake shows that one part belongs to the London Clay and the other to the Bagshots. Mr. Clement Reid continues his work on the Eocene deposits of

Hampshire, and some interesting notes are promised by the Director-General in his next Report.

The study of the Drift series has made steady progress, some 2,711 miles of boundary having been traced, mainly in East Cheshire, South Wales, and the Isle of Man. Mr. De Rance has noted the character and position of some 500 boulders, from two to twelve feet in length, in East Cheshire, and the information obtained has been supplied to the British Association Committee on Erratic Blocks.

In the Petrographical laboratory the main point of interest is the determination by Mr. Teall of the mineral Riebeckite as a normal constituent of the granophyre of Meall Dearg.

In Scotland, progress has been made in the mapping of Sutherland and Skye, Caithness, Aberdeenshire, and Banff, while important work has been accomplished in Kincardineshire and Forfar, Arran and Cantyre. Well-marked fossils have been determined in the shaly Cambrian series overlying the quartzites of the N.W. Highlands, which have allowed the positive fixation of the true Cambrian age of these beds. From the locality of Meal Gubhais, near Loch Maree, *Acrothele cadusia* and fragments of *Olenellus* rewarded the diligent search of Messrs. Horne, Gunn, and Clough. A collector, Mr. Maconochie, was accordingly directed to search the spot and a large series of specimens was obtained, and is now being worked out by Mr. Horne.

Mr. H. B. Woodward has completed his survey of the Jurassic rocks, with their associated Tertiary Igneous rocks, of Raasay, and a detailed section from below the Lias to the Great Oolite has now been obtained. It is interesting to note that the quartzite summits of the region north of Strathcarron retain their ice-worn character still so fresh that their polished surfaces, plunging steeply down the mountain sides, can hardly be walked upon. When wet, these glistening sheets of rock shine like polished marble, and show the fine striation impressed on them by the ice-sheet. Each larger valley among these mountains has its lateral and terminal moraines marking the positions of the later glaciers.

In Ireland the revision of the Metamorphic areas has been steadily progressing. Mr. Kilroe finds no break between the Croagh Patrick quartzite and the Llandovery strata, and it is therefore difficult to resist the conclusion that it must be of Upper Silurian age. But on this point the Director-General advises caution until the ground to the south has been exhaustively examined. In Palæontology, Professor Sollas has determined the existence of a new fossil (*Pucksia machenryi*) in a narrow band of slate, which had previously yielded *Oldhamia* and some spherical bodies resembling Radiolaria.

The Report concludes with some notes on improvements and accessions to the Museum of Economic Geology, and a return of work done by the individual surveyors attached to the Geological Survey.

THE half-yearly number of *Timehri* (the journal of the Royal Agricultural and Commercial Society of British Guiana), just received (vol. viii., pt. i.), contains several interesting and readable articles. The first, on Guiana Orchids, is by the editor, Mr. James Rodway, whose articles on the Guiana forest are known to readers of NATURAL SCIENCE. Valuable hints are given on cultivation in the tropics, for orchid houses seem as necessary in Georgetown as in a European

climate. Only in the wind-protected, saturated atmosphere of the forest are most of the species at home. A paper by Mr. Llewellyn Jones, entitled "A Few Popular Facts About Diffusion," will interest sugar growers; as will also "Some Enemies of our Canefields," and "Cost of Sugar Production in British Guiana."

The annual report of the Missouri Botanical Garden, which is very nicely got up, and is illustrated with 41 plates, contains reports for the past year, anniversary publications, and scientific papers. The anniversary publications are the fourth annual flower sermon, to defray which there is a bequest of $200, and the proceedings at the fourth annual banquet of the trustees ($969·55). Would not an abstract of the sermon have expressed all that was necessary? Of the scientific papers several have been issued in advance as "reprints," namely, "A Study of the Venation of the Species of Salix" (Glatfelter); "Bibliography of the Tannoids" (Bay); "Sugar Maples and Maples in Winter" (Trelease); "North American Species of *Gayophytum* and *Boisduvalia*" (Trelease). The others include Phenological Notes for 1892 and 1893, and a note on the emergence of *Pronuba* from the Yucca Capsules, by J. C. Whitten, and a list of plants collected in South-eastern Missouri. Among the plates, which are a marked feature of the book, are several very nice views in the gardens.

We have received a useful little pamphlet of twenty-four pages entitled "Brief Notes on the Physical and Chemical Properties of Soils," by R. Warington, F.R.S. (London: Chapman & Hall, 1894. Price 1s.). The subject is so slightly treated in most English text-books that it is intended to supply a temporary want, enabling teachers of classes in agriculture to treat the properties of soils more exhaustively than usual.

We have received nos. 1—4 of *Nederlandsch Koloniaal Centraalblad*, a publication devoted to the scientific literature of the Netherland Indies. Dr. Boerlage edits Botany; Dr. Max Weber, Zoology; Dr. Wichmann, Geology; F. Blumentritt, the Philippines; Dr. Van den Burg, Medicine; H. F. C. ten Kate, Anthropology; while C. M. Pleyte Wzn is the general editor. The publication, if kept up-to-date, cannot fail to be of the highest importance to dwellers in Malaya. The journal is published by Brill, of Leiden, and costs 2 fl. 50 per annum.

We have also received "Rainmaking and Sunshine," by John Collinson (London: Swan Sonnenschein, 1894. Price 3s. 6d.). The work scarcely falls within the domain of Natural Science, and we are impressed with the appropriateness of the author's motto:—"Man's knowledge is as a rivulet; his ignorance as the sea."

Messrs. Dulau & Co. have issued part xxxv. of their useful Catalogue of Zoological and Palæontological Works. More than fifty pages are occupied with a list of works relating to Anthropology and Ethnography.

OBITUARY.

WILLIAM TOPLEY, F.R.S.

Born 1841. Died 1894.

THE sad news of the death of William Topley has come as a surprise to his many friends. He had attended the Zurich meeting of the International Congress, and, full of interest and energy, he had proceeded to Algiers. There he was seized with illness, and on returning to his home at Croydon, it was found that gastritis had developed, a disease from which he never recovered. Mr. Topley, who was born at Greenwich, received his scientific training at the Royal School of Mines. In 1862 he was appointed an Assistant-Geologist on the Geological Survey of Great Britain, and six years later was promoted to the rank of geologist. Commencing field-work in the Wealden area, he became specially interested in the Cretaceous and Neocomian strata and in the problems connected with the sculpture of the scenery. His important memoir on the Geology of the Weald, and the paper (written jointly with Dr. Le Neve Foster) on the Medway Gravels and Denudation of the Weald, were results of this early work. In the meanwhile, and before the Wealden memoir was written, Mr. Topley was transferred to the far north of England, to take part in the survey of the coal-field of Durham and Northumberland. Later on, in 1880, he was appointed to superintend the publication of maps and memoirs at the Geological Survey office in Jermyn Street, and on the retirement of Mr. Best, in 1893, Mr. Topley took entire charge of the office.

Always full of work, and seemingly untiring in energy, yet he never published very much. The effects of denudation on anticlines and synclines, and the intrusive character of the Whin Sill, were topics he discussed from his experience in the north. He took an active part in the publication of the *Geological Record*, fulfilling for a time the arduous position of editor. He served for many years as one of the secretaries of Section C. at the British Association, and prepared for that body reports on Gold, on Coast Erosion, and on National Geological Surveys. He was one of the secretaries of the

International Geological Congress in 1888, when the meeting was held in London. Again, whether serving on the councils of the Geological Society or of the Geologists' Association, his services were in frequent request. He filled the office of President to the Association in 1885–86, and on many an occasion he acted as guide during the excursions of the members. Among these societies, as on the Geological Survey, his loss will for long be keenly felt, for he was ever ready with help and information. In the economic applications of geology, Mr. Topley was especially interested. The subject of the Channel Tunnel, the sub-Wealden Boring, and the underground distribution of Palæozoic rocks in the south-east of England engaged his attention. So also in questions of water-supply and of the occurrence of petroleum he was a recognised authority; and with regard to the water-supply of the metropolis he gave important evidence which was published by the late Royal Commission appointed to deal with the question.

Mr. Topley was Examiner in Geology to the Durham University and to the Science and Art Department. He was elected a Fellow of the Royal Society in 1888.

GUSTAVE HONORÉ COTTEAU.

Born 1828. Died August 10, 1894.

We deeply regret to have to record the death of the oldest and most respected member of the School of Systematic Echinologists—Gustave Honoré Cotteau. Mr. Cotteau was a judge in the Department of l'Yonne. In the intervals of leisure of a long and busy life, he was able to study the fossil Sea Urchins, and to describe probably more new species than any of his contemporaries. Fossil Echinoids from all parts of the world were sent to him at Auxerre to be determined and described. He took up the work of Desor when the author of the great "Synopsis des Echinides fossiles" passed away. For over 30 years he has been the greatest authority on his group. His work has been characterised by the most exemplary care; his diagnoses are models of close and detailed description. Some of the younger palæontologists may have thought he was inclined to attach more value than was necessary to trifling variations, but his determinations were always accepted with the most implicit faith owing to the care he bestowed upon his specimens and the love he put into his work. The list of his memoirs and papers is a very lengthy one, and among them it is difficult to pick out any for special mention. They are all so good. He was a Member of the French Institut, a Foreign Member of the Geological Society, and an honorary member of numerous societies who honoured themselves by honouring him.

The death is also announced of Dr. Paul Albrecht, lately of Hamburg, formerly of Brussels. He made numerous small contributions to Comparative Anatomy, and a few — such as his discovery of the pro-atlas in the vertebrate skeleton — are of importance.

Botanists, among others, will regret the death of General Robson Benson, who did some excellent work in connection with the Botanic Gardens at Rangoon and Madras.

Professor Pringsheim of Berlin was a botanist of eminence, who has also recently passed away.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Mr. C. L. Griesbach has been appointed the successor to the Directorship of the Geological Survey of India, in the room of Dr. William King, retired.

Mr. Charles L. Edwards, lately of the University of Texas, has been elected Professor of Biology in the University of Cincinnati, Ohio.

The fine collection of fossil Echinoids that belonged to the late Gustave Cotteau has been presented to the Ecole des Mines, Paris.

According to the *American Naturalist*, the American Museum of Natural History has organised an expedition, under the direction of Professor Rudolph Weber, to make collections in, and a scientific exploration of, the Island of Sumatra.

Mr. W. Holman Hunt has been appointed the Romanes Lecturer for 1895. Though Mr. Holman Hunt is best known in the regions of pure art, his presidential address to the Sunday Society, quoted in Natural Science, vol. v., p. 15, showed him to have a true sympathy with science.

Dr. James E. Talmage has been appointed to a newly-founded Professorship of Geology in the University of Utah. The chair has been endowed with a sum of $60,000 by the Salt Lake Literary and Scientific Association, and the Deseret Museum, belonging to this Association, has been placed at the disposal of the University.

At the annual conversazione of the Chester Society of Natural Science on October 8, the Kingsley Medal was awarded to the President, Dr. Henry Dobie, whose first contribution to science was published so long ago as 1849, and related to the minute structure of voluntary muscular fibre. In 1850 and 1851, Dr. Dobie published some observations on the cilia in the sponge *Grantia*, and he has subsequently made contributions to our knowledge of the rotifers.

The cases for the Barrande Collection of Lower Palæozoic Fossils in the Royal Bohemian Museum, Prague, are now fixed, and more than 20,000 specimens are already mounted for exhibition. Dr. Anton Fritsch, the director, estimates that the arrangement of the entire collection will occupy nearly a year. The Barrande Fund is at present being devoted to the publication of a memoir on Graptolites, by Dr. Perner.

The Museum of La Plata will shortly issue the second part of Mr. Lydekker's memoir on the Fossil Vertebrata of the Argentine Republic. It will be illustrated by over sixty folio plates, and deal chiefly with the Edentate Mammals. Mr. Lydekker returned to England last month.

We are pleased to learn that the Essex Field Club's petition with respect to the formation of an Epping Forest Museum has been granted by the Conservators, and active work will be commenced as soon as certain legal formalities have been complied with. Meanwhile, contributions of suitable specimens, and offers of loans of same, both for the Forest Museum and for the Central Museum at Chelmsford, will be gratefully received by the hon. secretaries, Messrs. W. and B. G. Cole, Buckhurst Hill, Essex.

The Norwich Castle Museum was opened by the Duke and Duchess of York on October 23. Much progress had already been made in the arrangement of the collections, and the general public were freely admitted three days afterwards. The *Norwich Mercury* celebrated the event by issuing an admirably illustrated supplement on October 20, descriptive of the transformation of the old prison into a museum. An earlier description of the new institution, by Dr. Henry Woodward, appeared in Natural Science for November, 1892.

Mr. Sauvinet, an assistant at the Natural History Museum of Paris, left on October 5 for Mecheria, in the Province of Oran, Algeria, to obtain antelopes and mouflons for the Jardin des Plantes. He will probably also obtain for the Museum some animals of the Sahara.

The same Museum has recently received from Mr. A. Boucard the valuable collection of birds that formed the basis of that gentleman's "Catalogus Avium," published in London in 1876. The collection consists of 25,000 specimens, representing over 7,000 species.

The different Alpine Clubs are doing good work in the study of the former extension of the glaciers in their districts. One of the latest of the reports is issued by the Italian Club, and contains a detailed survey by Professor Sacco, of Turin, of the old moraines in the northern Apennines. The lower margin of these moraines seldom descends below 700 metres, though in one valley in the Maritime Alps the glaciation reaches to within 500 metres of the sea level. In a few instances moraines have been recorded at lower levels in northern Italy; but Professor Sacco points out that moraine material washed down by rain or rivers has sometimes been taken to be true glacier deposit. It is often one of the most difficult questions for a geologist to decide, whether a moraine is truly in the position in which it was left by the ice, or has been washed down and reconstructed, perhaps at a level a thousand feet lower.

The "Return of the Income and Expenditure of the British Museum" for the year ending March 31, 1894, has just been issued. Among the acquisitions calling for special notice are the Pascoe and the Stainton collections of insects. The former of these contains about 49,000 specimens, of which no less than 3,000 are types. The Stainton collection consists of Lepidoptera, and is accompanied by a valuable series of original drawings of the larvæ of Micro-lepidoptera. Eighteen thousand specimens of Coleoptera have been received from Messrs. F. D. Godman and Osbert Salvin, being a further instalment of the rich collections acquired by the editors of the *Biologia Centrali-Americana*, which they are generous enough to place in the public collections. Messrs. Godman and Salvin have also enriched the Bird-room by handing over the remainder of their collections of bird-skins, consisting of 11,704 specimens, as well as 8,000 skins of birds from Mexico and 1,267 eggs of American species.

A series of 510 specimens of Cornish minerals have been presented to the Museum by Mr. J. C. Williams, M.P., many of which were obtained from mines now closed, and consequently inaccessible. The original drawings of most of the late Sir Richard Owen's discoveries have, through the kindness of the family, also found a resting place in the Museum.

A healthy record of work done towards the cataloguing of the vast collections is reported, no less than five volumes being listed. These comprise two on Birds, one on Snakes, one on Lepidoptera, and one on Corals.

From the special report devoted to the Department of Zoology, we learn that the whole of the series of Birds' eggs, some 47,000, has been arranged by Mr. Seebohm, and is now accessible. Six hundred and eighty type-specimens of land and fresh-water shells from the Morelet collection have been purchased and incorporated with the general series, and the arrangement of the Scorpions is nearly finished. The interesting groups of British Birds have been increased by three, viz., Greater Black-backed Gull, Dotterel, and Greenfinch, and a large series of models illustrating the life-history of insects has been placed in the insect gallery. No less than 130,185 specimens were received during the year, a number never before reached in the history of the Museum.

Turning to the Department of Geology, the chief acquisitions are the skull of *Megaladapis* from Madagascar (*see* NATURAL SCIENCE, vol. iv., p. 243), and the exquisite and perfect skeleton of a young Plesiosaur from the Oxford Clay of Fletton, near Peterborough. This, perhaps the most beautiful example of *Plesiosaurus* known, has been articulated so as to permit of the detachment of any single bone, and has been placed in a special case in the Reptilian Gallery.

In the Department of Mineralogy, the principal addition to the collection is that of the Williams Minerals above mentioned. The fine series of Meteorites has been increased by sixteen specimens, of which as many as ten are new to the collection. A series of 369 rock specimens, and microscopical sections of the same from the Allport collection, have also been secured.

No less than 17,677 specimens of plants have been incorporated into the collections of the Botanical Department, but the principal additions in Botany were the Deby Diatoms, comprising about 30,000 named slides, and the Jenner Herbarium of Algæ, containing 6,000 specimens. From the above sketch, the extraordinary growth of the Museum collections must be apparent to the least observant, and we hope soon to hear of extensions to the buildings, else the old Bloomsbury trouble must come back again.

As we go to press, we are informed by a correspondent that Mr. E. J. Bles, the Director of the Marine Biological Station at Plymouth, and Dr. G. H. Fowler, the Honorary Secretary of the Marine Biological Association of the United Kingdom, have tendered their resignations.

THE Rolleston Memorial Prize, for original work in biology accomplished by graduates of Oxford or Cambridge, has been adjudged to Messrs. M. S. Pembrey, of Christ Church, and E. S. Goodrich, of Merton College, both of Oxford, whose merit was considered equal.

CORRESPONDENCE.

The Bird's Foot.

I fear, from the courteous criticism of Mr. Mitchell in the September number of Natural Science, that I did not express myself clearly in my note on "The Bird's Foot," but if he will take the trouble to look over the article again, I think he will find that my intention was not to controvert Mr. Finn's proposition, but rather to maintain it. My conclusion in regard to the arrangement of the tendons was simply that the three-toed birds must have lost their fourth toe before its tendon became differentiated from the others, and this, it seems to me, is quite different from saying that birds "do not show evidence of the modification of their foot from a specialised four-toed foot."

If the deep flexors of a four-toed foot were entirely free from one another—as in the Passeres—and that bird were to lose the inner toe from disuse, would not the tendon of the flexor longus hallucis become aborted also for lack of something to pull upon? I think it would, but unfortunately we have no passerine or hummer minus a hind toe to settle the question. As to the distinctness of the muscles of the flexor perforans digitorum and flexor longus hallucis, if deductive reasoning is worth anything, we are justified in saying that these muscles are completely differentiated before the tendons, since all the more generalised birds are synpelmous while the flexor muscles are quite distinct.

In *Pterocles* and *Chauna*—there should, by the way, have been a query after the words *Palamedea* also, because I was not acquainted with its foot—it is to be noted that while the slip to the first digit is lacking, the flexor longus hallucis still meets with resistance from its vincular connection with the other tendons, and is thereby probably prevented from becoming abortive. Is there not a reason, based on habit, why *Palamedea* still retains the slip to the first digit? If I remember aright *Palamedea* is a bird of the forest region, and *Chauna* more a resident of the pampas, and does not *Palamedea* sometimes perch?

Turning now to the question of flight, while I cheerfully subscribe to the proposition that birds which fly well *to start with* will gain increased power of flight by ability to rest on the surface of the water, I protest that this does not furnish evidence that flying birds are ever derived from those which splash along the surface. In this connection it would be unfair not to speak of the habits of young ducks, suggestively named flappers by English writers on field sports, and not to mention the little Hoactzin which swims and dives well, although it does not, so far as I am aware, pass through any splashing stage. On the other hand, the difficulty experienced by the majority of the ducks in rising from the water in a calm, or down wind, is a most powerful argument against flight having been *acquired* in this manner. Right here let me say that Mr. Finn's suggestion in the foot-note on p. 211 seems extremely good. I am quite ready to believe that flight originated by jumping *down*, but not by jumping *up*. It should be remembered that the pluvialine relatives of the Gull fly remarkably well, and that the Gull is but an indifferent swimmer, gathering its food along the shore or at the *surface* of the water, not beneath it. The broad-winged Pelicans, too, are surface swimmers, not very good ones either, and sit high upon the water like the Caravels of old, instead of ploughing through it after the manner of a torpedo boat, as do their relatives the Cormorants and Snake Birds. In

fact, I am quite willing to rest my case on the habits of the various members of the order Steganopodes; the better they swim, the worse they fly, and *vice versâ*. As for the Frigate Bird, so far as habits go, he can scarcely be deemed aquatic at all, but rather "a creature of the upper air," "never alighting on land nor resting upon the water."

There are still many points of interest in the tendons of the bird's foot, and "one of these days" I hope to take up the subject again.

FREDERIC A. LUCAS.

IF in my note in the September number I did not rightly apprehend Mr. Lucas's position, the misapprehension is the less to be regretted as it has called forth another interesting note from him. It seems that we are in agreement on the immediate point, and differ only on a point on which zoologists may well expect divergence. An argument involving the terms "effect of disuse," "rudiment," and "phylogeny," cannot be entered on in the affix to a letter, and I am content for the present to accept Mr. Lucas's re-statement of his position. P. C. M.

DISPERSAL OF THE NUTLETS IN A LABIATE.

A PROPOS of Miss Pertz's interesting paper on the "Dispersal of Nutlets in certain Labiates," a member of the British Association asked, at the time the paper was read, what was the advantage to the plant of the reflexed hairs in the calyx of *Teucrium Scorodonia*; and he drew on the blackboard a section of the calyx, showing the hairs well reflexed. No answer was offered at the time. A solution at once occurred to me, that the hairs were of use in preventing the nutlets from dropping out, as soon as mature, at the foot of the plant; an impediment, such as a ring of hairs, would retain the ripened nutlets in the calyx till a strong enough wind arose to overcome the resistance, and flirt the nutlets out to a distance from the parent plants. The puzzle was, why should the hairs, which are rather stiff when mature, be *reflexed*? a position likely to defeat the supposed object.

After examining several plants this autumn of *Teucrium Scorodonia*, I do not find that the hairs in the calyx are reflexed in the living plant. When the corolla has fallen, the hairs, which hitherto have been pressed erect between the corolla and calyx tubes, begin to descend, moving very gradually through an arc of 90 degrees, till they finally assume a horizontal position; their curved, flexible tips meeting and interlacing just above the maturing nutlets. When these are matured, the ring of hairs is just so rigid that a violent shaking of the plant is required to eject them; and it is obvious that the force which is sufficient to do this will be enough to throw them to some distance. It thus appears that the obstruction offered by the spreading hairs in the calyx actually assists in the wider dispersal of the nutlets; while the slight upward curve of the four teeth forming the lower lip of the calyx would, perhaps, have a similar tendency, giving an upward direction to the flight of the nutlets, when shot out by any violent motion. A free course is allowed them, so far as the upper lip of the calyx is concerned; for in this species it is remarkably bent back, almost at right angles to the calyx-tube.

E. F. LINTON.

THE MUSEUMS' ASSOCIATION.

As one of the executive of the Dublin local committee, I confess I read with indignation the note "Science at a Picnic" in the August number of NATURAL SCIENCE. Such a heading, savouring of the up-to-date style of a branch of London journalism, sounds inviting. The correcting reply from the Association Secretaries in this month's number (p. 319) shows how inaccurate and misleading the statements made under the above title were, and one would have been content to say no more had not you, as Editor, practically repeated (p. 320) the earlier statements, with now accompanying and apparently justifying explanations. The meeting lasted *four* days, not three, as stated. The President's address, though printed in the August number as the most important contribution to the meeting, is not admitted as part of the Association proceedings. The only day of the four not as full as it could well be of

actual museum work was Thursday, when an excursion was arranged, not simply to a waterfall (p. 320), but to the mansion and estates of one of the most enlightened of Irish noblemen, Lord Powerscourt, F.R.S., whose art collection, viewed with great interest by the art curators present, is, I am told, one of the finest. Two papers were shortened, one was my own, taken last, and shortened at my own request, to make way for the other "burked" (?) paper, which was, though contributed by an able member of the Association, of an extremely discursive nature, and was at the end of half-an-hour "no forrarder." Discussion on the papers was not seriously curtailed. Once or twice it was invited, and was not forthcoming.

As an Englishman engaged in science work here, I wish to protest against the countenance your note gives to the too prevalent opinion that Irishmen are not capable of sustained and earnest work.

Dublin. T. Johnson.

[We can assure Professor Johnson that, neither in our original note nor in our subsequent defence, did we intend to cast any aspersions on the Irish character, or to slight the labours of the Dublin local committee. Our views as to the mode of conducting a meeting of this kind may not meet with universal approval; there is room for argument. Our statements, however, were based partly on the official programme of the Association, which we knew was not absolutely adhered to, and partly on information received from members of the Association, present throughout the meeting, whose good faith we have no reason to doubt. We wish so well to the Museums' Association that we should regret indeed to arouse the indignation of its members against us; but under the circumstances we can hardly say more than that we *hope* we were wrong.—Ed.]

The Glacial Period and Primitive Man.

On examining the evidence for the immense antiquity often attributed to Man, it is apparent that there is a great difference between the pre-Glacial and the post-Glacial. The post-Glacial evidence is of a varied character, comprising not only stone weapons but other implements, rude drawings, charred and split bones, etc. On the other hand, the pre-Glacial evidence entirely consists of rudely-worked flints. In fact, the deniers of human antiquity, when confronted with the post-Glacial evidence, cannot deny its human origin, so take refuge in denying its antiquity; while, when opposed to the pre-Glacial, they admit the antiquity, but deny the human character.

Now it can hardly fail to be remarked that during the immense period for which Man is often supposed to have existed before the Glacial Period, the progress of the species was very small—in fact, compared with its subsequent advance, almost nil; while, during the far shorter time that has elapsed since, progress has been continuous and rapid.

Accordingly, I venture to suggest the following hypothesis. Man, that is the intelligent and progressive animal known as Man, dates only from the Glacial Period. Before this there was no true man, but only a very intelligent anthropoid, surpassing those of the present day, being able to use properly-shaped stones as weapons, and when such stones in their natural state were inconveniently formed, to break off fragments, and so reduce them to a convenient size and shape. Probably it also knew the use of fire. Furthermore, it resembled Man, physically, much more closely than any existing species does. Nevertheless, it was in all essential respects only an ape.

The severity of the climate at the commencement of the Glacial Period caused a great mortality among this species; only individuals survived who were able to adapt themselves in some degree to the changed conditions. Under this pressure the species changed rapidly. The development was, of course, mainly in intelligence and adaptivity; especially intelligence in dealing with fire, now becoming a vital necessity. Strength and activity, being of secondary importance, were no longer kept up by Natural Selection, and so retrograded, either through the accumulated

effect of disuse, or economy of growth; or perhaps the possessors of extreme strength and activity were more rash, combative, and foolhardy, and so more liable to be killed. There was also a change in certain physical characteristics, consequent on the enforced change in habits. Thus the change from ape to man was, so to speak, sudden, the transformation being effected probably in a few thousand years.

The question of the unity or plurality of the origin of Man was at one time a good deal debated. I am not sure that, upon the hypothesis of development, the words have precisely the same meaning as upon the older hypothesis of a special creation; for we now regard the whole race as undergoing a gradual transformation, and do not regard the new species as descended from a single ancestral pair. Nevertheless, the words can be given a meaning; either (*a*) did the evolution take place in one spot and at one time, or in different places and at different times? or (*b*) was the evolution from a single "species," or from more than one?

In the latter sense, I am inclined to believe in the unity of origin. It seems, however, just possible that, while one race arose as above suggested, another may have arisen in the tropics, say in Equatorial Africa, where there was no Glacial Epoch. In consequence, it was deficient in intelligence and essentially unprogressive. The existing races are the result of the fusion in varying proportions of these two.

I think it more probable, however, that the whole race was, in a sense, imprisoned by the Glacial Epoch, making locomotion difficult and dangerous. When the first interglacial period set in, it was liberated, and in consequence spread down to the tropics, which were thus peopled with Man, but a very primitive form of Man. The tropical regions underwent no further glaciation, and consequently their inhabitants were not compelled to improve; the portion of the same race who stayed at home underwent another glaciation, and were in consequence further developed. When mild conditions again came round, this more highly-developed race spread towards the tropics, driving their predecessors before them, and to a certain extent amalgamating with them. In this way, successive waves—each more highly developed than the former—spread out from the glaciated regions towards the equator on the occasion of each interglacial period. So we might expect to find that the lowest type has been driven into the equatorial forests. Here it would hold its ground, being nearer to the anthropoid ancestor, and therefore better suited to the environment than the succeeding races.

It may be objected that a race which had passed through a glacial climate, and to a certain extent acclimatised itself, would probably find even the interglacial climate too warm, and would, therefore, be more likely to spread towards the pole than the equator. I fancy that the first spreadings were more probably towards the pole. It was the approach of the second glaciation that drove a portion toward the equator. Those who had spread toward the pole were probably killed out by the extreme severity of the climate when glaciation again supervened.

To guard against possible misconception, I may remark that I do not attribute the transformation *merely* to the severe conditions; the intellectual transformation was caused principally by the varying conditions, necessitating a certain power of adaptation. Had the severe conditions prevailed unchanged until now, after the first transformation the organism would have adapted itself to the new surroundings, and, further power of adaptation being unnecessary, would have disappeared. The Esquimaux show what unvarying severity of climate can effect; and had the cold of the Glacial Period continued unchanged, the human race would probably be lower than the Esquimaux. It seems to me that, even at the present day, we can see that in races which have long (2,000 years or more) inhabited a region with unchanging climate, progressiveness and the power of assimilating new ideas die out.

J. R. Holt.

The Erect Posture of Man.

I have several times seen it stated that the erect attitude of man constitutes a very serious difficulty in the way of those who believe that only

Natural Selection has been the cause of his development. The matter is not mended by showing that the erect position is better than the crawling, because it is necessary for Natural Selection, not only to show that the final result is better than the initial, but that every step on the road carried an advantage with it. The problem is certainly difficult, but I do not think it is insuperable, and I have a suggestion to offer which may, perhaps, lead to a partial explanation.

The idea was suggested to me by watching mungooses in India; when these animals wish to look around them, they sit up like a dog begging, bringing the head as high as possible. Now, if we suppose that in the commencement of the Glacial Epoch, the anthropoid was liable to attack, either from carnivora, as famished wolves, or from his own species, evidently extreme circumspection would be requisite; a single moment of inattention might prove fatal. Evidently, the result would be that those individuals who were able to carry the head aloft persistently would have an advantage over those who could only raise it up now and then.

This may account for the head being carried high, and the back being straight, but by itself it will not account for the disuse of the fore-limbs in locomotion; it would be more likely to lead to excessive lengthening of the arms, so that while the back was straight, the hands could be rested on the ground.

I have postulated that this anthropoid had sufficient intelligence to use stones as weapons; now, stooping to pick up a stone would necessitate being off guard for a moment, during which a sudden attack might be made; moreover, if the ground was frozen hard, detaching a stone would take time, and necessitate a prolonged period of helplessness, during which an attack would be disastrous; evidently it would be desirable for the animal to carry a suitable stone *with* it, by throwing which the adversary might be, if not killed, momentarily disabled, and time gained to pick up another stone. It would be difficult or nearly impossible to use the arm belonging to the hand carrying the stone in locomotion, besides which it might be desirable to have it free in case of sudden attack. So the normal progression would probably be nearly erect on the hind limbs, with occasional assistance from the arm not used in carrying the stone.

In fighting, or in hurling missiles, extreme firmness on the legs would be desirable; the slightest stumble might prove fatal. Now, those individuals who dispensed with the assistance of the arm as much as possible, and used the legs only, would come to have stronger legs and be steadier on them than those who made much use of the arm in locomotion; they would consequently have an advantage in the struggle for existence.

If we admit the possible transmission of acquired characters, this steadiness on the legs might be transmitted; but it is not necessary to assume this. My argument is that those individuals who had a congenital dislike for using the arm in locomotion, or preference for the unaided use of the legs, would become steadier, and so have an advantage over their fellows; and it is this partiality for using the legs only which I suppose to be transmitted.

The erect posture, and the habit of using only the legs in locomotion, being acquired, there would no longer be any advantage in having the arms long enough to touch the ground. Now, in hurling missiles, too long an arm would be a decided disadvantage, as it would make the animal liable to over-balance himself. Too short an arm would be a disadvantage, obviously, too. The arm would, therefore, shorten to some extent, until the best length for throwing stones had been attained. The advantages of a firm grasp, either in throwing or in using weapons, would also lead to modifications in the form of the hand.

I have laid considerable stress on the advantage of carrying a stone, so as to be at once ready for attack. Remembering that this missile must have frequently failed to hit its mark, it may be supposed that I have laid too much stress on a very slight advantage. But I suppose the struggle for existence to be terribly severe. Food was scarce, and therefore the anthropoid had to venture far afield to seek it, or starve. Trees were scarce, so he could not seek safety in climbing. Probably the change from a frugivorous to an omnivorous diet was contemporaneous. After having killed his assailant, he must often have been too terrified and exhausted to

search for fruits, and have eaten the dead body. Judging from our present animals, the change from a vegetable to an animal diet must have been very easy. Most of our frugivora, as squirrels, etc., will eat insects, and not unfrequently prefer them.

I have supposed the change to have taken place in the Glacial Period, but obviously this is not essential. The essential points are, (1) fierce struggle for existence, (2) liability to sudden attack, (3) recourse to trees cut off.

Most probably this hypothesis will prove untenable on criticism, but I think I have shown that the erect posture is not an insuperable stumbling-block, that at least it is possible to *imagine* ways in which it might have come about.

6 Harrington Street, Dublin. J. R. Holt.

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, 5 John Street, Bedford Row, London, W.C.

All communications for the Publishers *to be addressed to* Macmillan and Co., 29 *Bedford Street, Strand, London, W.C.*

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CHANGE OF ADDRESS.

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NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 34. Vol. V. DECEMBER, 1894.

NOTES AND COMMENTS.

The Scots Fishery Board.

IN our September number we were able to announce that Mr. John Murray, of the "Challenger," was very strongly supported by leading scientific men for the post of Chairman of the Scots Fishery Board, in succession to the late Mr. Peter Esslemont. The appointment is made by the Crown, and no doubt the recommendation of the Scots Secretary is the important factor in determining the decision. Mr. Murray seemed peculiarly marked out for the post, from his special attainments in connection with marine biology, and from his long and capable service under Government in connection with the "Challenger" expedition and the "Challenger" reports. Most of our readers are now aware that Mr. Angus Sutherland, the Liberal Member of Parliament for Sutherlandshire, has been appointed chairman, and has in consequence vacated what may be regarded as a safe Government seat.

As this seems a post which, to the great advantage of the State, might be filled by a man of special distinction in science, it is worth while going into the history and nature of the Board. Like so many excellent Scots institutions, it dates back in origin to a time long preceding the date at which Scotland began to enjoy the advantage of union with England. The first White Herring Fishery Board was founded in the 14th century, no doubt with a view to secure some benefit to the Crown from sea-fishery. The Board of British White Herring Fishery, the parent of the present Board, was established by 48 Geo. III., c. 110, and was dissolved in 1882 by the 45 and 46 Vict., c. 78, which established the present Fishery Board and is the basis of its action. The Act provided that the Board should consist of the sheriffs of three sheriffdoms, to be selected by the Crown, and six other members to be appointed by the Crown. The Crown nominates one member to be chairman, who has a salary of £800 per annum, and appoints the secretary,

who has a salary of £300–£400. The Secretary of State of the Home Department appoints the inspector of salmon fisheries, who has a salary of £600.

The Board have the appointment of all the other offices, such as the general inspector of sea fisheries, with salary of £300–£350, an assistant, with salary of £250–£280, and a number of minor appointments. £1,800 is given to the Board to spend in scientific investigation: they opened at Dunbar about twelve months ago their first fish-hatching establishment.

The Board have the general superintendence of the salmon fisheries of Scotland, and take such measures for their improvement as the funds under their administration allow. They are the only authorised authority for branding herrings, and from this source they derive an annual income of about £8,000. They look after the piers and quays under the authority of 5 Geo. IV., c. 64. Their expenditure under all heads amounts to about £28,000 a year. At the beginning of this year the only scientific member of the Board was Professor McIntosh, of St. Andrew's.

Here, then, is a great permanent department with most important duties and with a jurisdiction extending over all Scotland and the Northumberland coast. For the administrative and executive duties, the permanent staff and a board of intelligent persons selected for any reason whatever should suffice. But nothing is clearer than that the main object of expenditure should be the maintenance and improvement of Fisheries, now that the income is no mere wresting of taxes for the Crown. The regulation and repair of piers and quays, and commercial necessities like the branding of standard qualities, are matters within the intelligence of most. The times and seasons for protection, the regulation of trawling, the provision of bait, and above all the extension of fish-hatching stations are matters that can be solved only by trained scientific experts. Sheriffs, in Scotland, are necessarily learned men; Members of Parliament, no doubt, have special abilities and have the opportunity of being in touch with the practical needs of their fisher-constituents. But it is urgent and imperative that in this country, as in France and America and Russia, departments dealing with problems soluble only by science should be under the responsible control of scientific men.

This is no question of Mr. Angus Sutherland and Mr. John Murray: it depends upon no weighing of their personal capacities. The choice of individuals is best left in the hands of a responsible department, and many considerations unknown even to omniscient editors may select or reject an individual. The choice of the kind of individual should not be left to Government.

Science and the State.

For the selection, by Government, of the wrong kind of individuals for posts that should imply scientific knowledge in the holders,

we blame not Government, but men of science. In all matters touching State-craft the habit of these is to hold aloof superiorly, leaving mere politics to the common man, or to rush belated into a controversy, delaying compromise by new discharge of venom. The savant in his laboratory, ignorant of the bombardment of his city, is a figure that tickles the fancy, but should offend the judgment. The irruption into the Irish controversy of a great scientific man, since unhappily dead, and his easy pre-eminence among irreconcilable and turbulent partisans, was food for laughter and tears. Scientific men have no right to wait till controversies strike down to their elemental passions. They must take their part as citizens in daily conduct of the State. Otherwise, when the pinch comes, they will be unschooled in the diametrical opposition between political and scientific knowledge as bases of action. In scientific action the expedient depends upon the recognition of what may be certain among facts; in political action the expedient depends upon the recognition that no facts are certain.

Thus scientific men have succeeded in gaining a reputation, especially among politicians, for acting in the conduct of affairs either negligently or with virulent self-confidence; whereas the fact is that what error of this kind lies in them is no more frequent than the corresponding error of politicians in matters of science. The defects of the qualities of science may be want of prudence and conciliation: the defects of the qualities of politicians are want of decision and certainty.

Decision and certainty are absolutely called for in matters like the maintenance of fisheries. There is no room for compromise with nature: knowledge of nature is not opinion, but definite and inevitable fact, and again we must insist that scientific men must be appointed to posts involving scientific training and knowledge in the conduct of the duties.

But scientific men are to blame in another fashion. Professions like the Law, the Church, or Medicine are close corporations or trade unions, which take vigorous action to support their members. Partly for the advantage of their members, partly for the advantage of Law, or Medicine, or Religion, they secure that every post shall be held by a suitable member of their own bodies: that the untrained and inexpert, whatever their general ability may be, shall not be appointed.

With us there is nothing corresponding. Here is a field for the British Association: to form a committee that shall be an advisory and consultative board on all matters affecting the interests of Science, and the interests of scientific men. It must be a committee elected by a body large enough and representative enough to convey the imperative judgment of Science, and strong enough to secure that its advice shall be acted upon. Our English Government is no autocrat that can disregard considerations presented to it by weighty authorities. If retired naval and military officers or mere

members of a party occupy posts that should be held by scientific men, not Government but scientific men are to blame.

GEOLOGY IN QUEENSLAND.

WE do not know whether it is that geologists are more united, or that geology, from its obvious connection with the Deluge, has had the public ear for a larger period than other branches of science that should come within the cognisance of the State, but the recognition of geology by the State has resulted in more active work in geology, both in England and in the colonies, than in any other branch of science.

We have received the *Annual Progress Report* of the Geological Survey of Queensland for the year 1893, by Mr. R. L. Jack, the Government Geologist. The work of the year appears to have been much interrupted by the disastrous floods, which took place at Brisbane during the month of February, and which might have irretrievably injured the collections had not prompt measures been taken by those on the spot, and had not the officers out in the field been summoned home by telegraph.

Since that time of trouble the offices and the collections of the Survey have been removed from their temporary home in Elizabeth Street to a more centrally placed building, recently occupied by the Registrar for Titles' Department. Here the collections have been rearranged and exhibited by the two energetic and able Assistant Geologists, Messrs. W. H. Rands and A. G. Maitland. The result of this moving, and the greater available space for exhibition, appears to have been to increase the popularity and therefore the usefulness of the Museum. Even at the somewhat inconvenient premises in Elizabeth Street, the collections were daily visited by about twenty-one people; the attendance has now greatly increased.

The exhibits in the Museum are arranged under the four heads of Maps, Minerals, Rocks, and Fossils. The productions of the colony itself naturally occupy a prominent place, and, subordinate to a broad scientific classification, they are arranged in accordance with their geographical position, beginning in each case with the northernmost representative. The rocks of Queensland are now completely exhibited for the first time. The arrangement of the fossils naturally follows that adopted in Messrs. Etheridge and Jack's excellent work on "The Geology and Palæontology of Queensland and New Guinea."

In a rising and imperfectly surveyed country such as Queensland the officers of a Geological Survey have naturally little time to spare for what we at home dignify by the title of pure science. They are bound to consider the immediate need of the community that pays them, and to devote their attention to the practical and commercial side of geology. In fulfilling this duty, the Geological Survey of Queensland, though miserably small in numbers, and though suffer-

ing from the financial whirlwind that lately swept over Australia, acquits itself right well. The most important work of this nature has been the survey of the well-known goldfield at Charters Towers. A geological map of the field, showing the surface outcrops, was issued early in the present year; since then the underground structure has been carefully investigated by all the available strength of the Survey, and a fresh edition of the map, embodying this work, will shortly be published. It is safe to say that this will now be the best surveyed goldfield in Australia, and that the detailed maps will be of enormous scientific and practical value.

In addition to actual surveying and collecting, the officers of such a Survey are continually being asked for information by prospectors, miners, agriculturists, and others. And in our colonies, it must be remembered, the officers of the State have to behave far more as the servants of the Public than some of them think it necessary to do at home. The Geological Survey of Queensland has recently received many enquiries as to water supply, especially as to the probabilities of finding it through artesian borings. Information is carefully collected and again retailed, which, we doubt not, greatly benefits the enquirer. But, as all who have had anything to do with water supply know well enough, it is an exceedingly difficult matter to advise upon, in the absence of very full and precise information as to the structure of the surrounding country. The circumstances may all point to the likelihood of an artesian well being successful, but an unperceived fault may disarrange the strata and prove the ruin of the whole attempt. Consequently, before a survey can be of that practical service to the community which is hoped and demanded, it is necessary that the whole country be adequately mapped in as great detail as possible. This, of course, is not a task to be undertaken in a hurry or by rule of thumb; it demands slow collection and collation of facts and prolonged scientific study. Many of the steps necessary for the successful accomplishment of the task are not such as appeal to the uninstructed public, which only regards immediate results. We refer especially to the collection and study of fossils, and the correlation of strata by their means. All this, however, must be done, and men and money must be supplied. The experience of all countries has shown that the investment is one that eventually pays a large interest, and we sincerely trust that, for the sake of the rich and beautiful colony of Queensland, every facility will speedily be given to its Geological Survey to continue the work so worthily begun by Mr. R. L. Jack and his devoted assistants.

The U.S. Geological Survey.

The September-October number of the *Journal of Geology*, just to hand, contains some observations by Professor R. D. Salisbury, *à propos* of the work of the United States Geological Survey, which are very

germane to our remarks on the Queensland Survey in the preceding paragraph.

It appears that additional appropriation has been made, providing for the gauging of the water supply of the United States, and for the investigation of the artesian water areas. This was by no means before it was required; for the information constantly demanded from the Survey, concerning the water resources of the country, is far more than it is at present in a position to give. Under Major Powell's administration an excellent beginning was made, but it was only a beginning, and by no means sufficient knowledge was obtained to enable the Survey to solve the various problems that are continually presented for solution. The mere fact that so many applications are made to the Survey shows that the American public is at last awaking to the fact that a knowledge of geology may save them many thousands of dollars.

The hydrographic work of a Survey must be based not only on geology and topography, but on meteorological records. The variation in rainfall on the one hand, and in barometric pressure on the other, are potent factors in determining the annual or seasonal yield of any given district. Meteorological information may no doubt be obtained from the already constituted Weather Bureau of the U.S. Government; but, as was pointed out in our review of W. B. Clarke's "Climatology of Maryland" (Natural Science, vol. v., p. 148), the climate of a country is itself largely dependent on its topography and geology. Thus the whole work falls most naturally into the charge of the Geological Survey. We learn from the *Journal of Geology* that "the organisation of the department of hydrographic work under the present limited appropriation is under the charge of Mr. F. H. Newell, who is assisted by Mr. Arthur Davis. Both of these gentlemen are trained topographers, and have had long experience in such hydrographic work as is contemplated by the Survey. At present they are largely engaged upon the special study of the water supply of the great arid and semi-arid regions of the interior, employing local assistance wherever parties are found who are interested in the work. A large amount of this assistance is voluntary, so that they are able to obtain much more extensive results than would otherwise be possible with the resources at command. The railroads, especially, are giving much assistance by having their bridge-tenders read the river gauges that have been set up under the direction of Mr. Newell."

Mr. Salisbury also informs his readers that the Director of the U.S. Geological Survey proposes to establish a laboratory for the study of materials used for road-making. This, it is believed, will give a great impulse to the improvement of highways throughout the land.

Hitherto much money has been wasted owing to the employment of inappropriate road-metal, which produced roads dusty in summer and muddy in winter. "Inferior materials have sometimes been used, when, in the immediate vicinity, there were other materials which,

alone or in combination, would have produced a solid road-bed." "A large part of the country, including the greater portion of the Southern States, and some portions of the Mississippi Basin, has been thought to be essentially destitute of materials suitable for the construction of good roads. The inquiries that have been made by geologists have shown that in many places within these regions there are hidden deposits of gravel and other sorts of rock, which, when properly used, might give excellent highways, and that around the margin of this great area, often within the limits of convenient railway distribution, there are abundant supplies of rock well fitted for such use. It only remains to discover the supply of such stone as is cheapest and best for the use of each region. . . . . For more detailed information, it is proposed that the various road commissioners send to the Survey samples of such rocks and gravels in their immediate vicinities as are believed to be valuable for road construction."

A laboratory in connection with the Massachusetts Road Commission has already been established at Harvard University. It is, however, not thought advisable that each State should establish a laboratory, for this would lead to much duplication of work, and after a time little work would remain to be done. Moreover, "the results obtained by divers observers and methods would lack the unity which give [*sic*] a national value." An officer of the Survey will therefore be detailed to take charge of the investigation of road-metal in the Harvard laboratory, and the establishment of a national laboratory will be brought to the attention of Congress, and a request made for a suitable appropriation.

As we firmly believe in the truth of the remark once made by a wise man, that the status of a nation's civilisation may be estimated by its facilities of communication within its own borders, so we cordially approve of these ideas of the new Director of the U.S. Geological Survey, and hope that in his attack on Congress he will meet with the success that his efforts deserve.

A Geological Record.

Although, in its relation to the State, geology is active and vigorous, yet it exhibits at present symptoms of neglect of a matter vital to the progress of scientific work. Rumours are rife that records of geological literature will soon be as dead as the Dodo, by reason of the difficulty of getting supporters in sufficient numbers to make such ventures pay. Talkers there are in plenty, and agitators for Records of new kind and arrangement; but few there be sufficiently interested in such work to put their hand into their pocket and keep a Record going. Dagincourt's "Annuaire" has made a good fight, has filled a great gap, but it is not supported as it should be. Blake's "Annals," too, dies from the non-support of those who clamour for Records. But during all the births and deaths of Records as individuals, there has yet been one that has

weathered the storm, and that solely because it has been supported by the very life-blood of our own Geological Society. Commenced many years ago as a quarterly list of books received, under the auspices of Thomas Rupert Jones, then secretary to the Society, it has gone steadily on, recording year by year the contents of every serial and the name of every book and map that has been received into the library. This year it may still be found by the curious in the November number of the *Quarterly Journal of the Geological Society*—ignored by nine-tenths of geologists, greatly prized by the remaining few. For of all lists published, these are the only ones where you may find a complete record of the geological papers in any particular serial for something like twenty years. It is not too much to say that this is the most valuable publication that the Geological Society gives to its members in the course of the year. The "Additions to the Library," for so it is called, has, we believe, been compiled by the present librarian, Mr. William Rupert Jones, for many years, though we have never observed his name attached to it. The list just published, which covers the period July, 1893—June, 1894, consisting of 125 pages, exceeds its predecessor in length by 45 pages, and is a remarkable example of careful and painstaking recording. We trust the Geological Society, whatever it may do in the matter of records, will keep this portion of their publications intact, for it serves a purpose which no other record has ever served, in giving a continuous account of the contents of various serials which is of considerable utility and value.

A Universal Zoological Record.

In this vital matter of recording, a most important step in advance comes from America, and Minneapolis has practically begun what should have been done by London, Naples, or Berlin many years ago. This means that we have received from Mr. Clarke Barrows, of the University of Minnesota, certain printed catalogue slips on cards of the well-known size supplied by the Library Bureau of Boston for card-catalogues. The arrangement of one of these slips is here reproduced :—

Wilson, Edmund B.

1892 The Cell-Lineage of Nereis. A Contribution to the Cytogeny of the Annelid
July Body. 7 plates, 8 wd.cts. & 1 cytogenetic table.

Journal of Morphology, Vol. VI, Nr. 3, pp. 361-460 (480).

Abstract in—

O [punched if required.]

These cards are samples of what is intended eventually to be a complete card-catalogue of zoological literature arranged under authors' names. The beginnings of the undertaking are, however, small. It is proposed to begin the catalogue with the current volumes of the periodicals now available at the University of Minnesota, which only number forty-two, whereas the actual number of periodicals containing zoological papers is not less than a thousand. But arrangements will soon be made to get the titles of all other important zoological writings, so that the catalogue will be complete for all zoological literature not recorded in Carus and Engelmann's Bibliotheca Zoologica up to the end of 1861.

The great feature of the proposal, however, is not the completeness of the catalogue, but the fact that these cards will be issued to all who choose to subscribe for them, at a rate not exceeding a penny a slip, thus furnishing them with a continuous and up-to-date reference to all zoological literature. The advantage of this plan over all other systems is that the list is always kept in alphabetical order; or slips can be selected and arranged in any other order that the purchaser desires. Let us make for a moment the very desirable supposition that the promoters of the *Zoological Record* will subscribe for a set of slips. Then the editor could at once roughly sort out the large majority of these slips and hand them over to the recorders of the different groups. The remaining doubtful slips could be gone through by the editor or any competent zoologist with the aid of the original publications, and could thus be sorted according to the group or groups with which they dealt; duplicates could of course be purchased when required. By this simple means each recorder would be furnished at the start with his list of titles, and whether the list were complete or not it would at all events be known that it was complete for certain periodicals. Each recorder would thus have to refer only to the papers in his list, and would be spared the drudgery of wading through piles of profitless publications. Librarians also will be glad to be spared the trouble that authors' copies are continually giving them, since their subscription will get for them not merely the reference, but a slip all ready to insert in their card-catalogue. In short, this enterprise may do much to remedy the sad state of things described in the vigorous article by Mr. Stebbing contributed to our last number; and we venture to think it superior to the proposals that Mr. Stebbing has put forward there.

There is nothing very new in this plan. Similar suggestions were made in the correspondence columns of *Nature* during August and September, 1892, while on the continent Dr. H. H. Field seems to spend most of his time in advocating the establishment of a central zoological bureau to perform this very work.[1] The only thing that is new, and that is really astonishing, is that someone has at last made a beginning. We may for various reasons regret that

[1] See *Mém. Soc. Zool. France*, vii., p. 259: 1894.

it has been left to Minneapolis to show us the way, and we could wish that the headquarters of the movement had been in a city where there were a few more zoological publications than are to be found in the library of Minnesota University. But we must rejoice that the admirable methods of library cataloguing which have been brought to such perfection by the Americans are at length to be applied to the relief of the overweighted zoologist, and we must hope and strive that sufficient pecuniary support be forthcoming. If the undertaking be once fairly started, and if it acquire those proportions that are needed to make it really valuable, then we shall soon be wondering how on earth we ever managed to get along without its assistance. Those, and we trust they are many, who desire to aid this good work, should communicate with Mr. Clarke Barrows or Professor H. F. Nachtrieb at the University of Minnesota, Minneapolis, U.S.A.

The Linnean Society's Catalogue.

There is not a wide difference between Records of Literature and Catalogues of Libraries, and while we are on this subject we may well consider the strange case of the Linnean Society. For many years past, successive Councils of that learned body have struggled with the apparently hopeless task of bringing out a list of their books that shall supplant the old catalogue of 1866. Such a work should take certainly not more than one year to accomplish, but although there have been many rumours of its incubation, nothing has as yet appeared. We say "appeared" advisedly, because we have ourselves seen an abortive pamphlet which purported to be an attempt at a catalogue of the serials, but, so far as is known, that has never been distributed or sold by the Society. Surely years enough have passed since the publication of the last catalogue to make it worth the while of the Council to publish another, for the library contains an interesting series of books quite apart from the precious volumes which at one time formed part of the library of Linnæus himself, and many of which are almost if not quite unique in this country.

It may be urged in excuse that the officials of such a Society have enough to do to carry on its regular business, but there are many men who would be perfectly competent to make a catalogue, and who would be glad of such congenial occupation. It is curious how apathetic ordinary members of societies are: they are content with the pride of membership and with the idling of an occasional evening; so long as tea and coffee are served they do not care one jot what happens with regard to the general management. But surely there must be many members of the Linnean who are sufficiently alive to the importance of knowing what the resources of their library are, and it is surprising that they do not insist on a catalogue being published.

Possibly by the next anniversary meeting (May 24, 1895), some Fellow may be goaded into asking for what may be an explanation

but can be no excuse. In the meantime, it would be quite possible to make a catalogue. The members of such a Society as the Linnean, with a historic past, should regard themselves as trustees for the hopes of the original founders, and they should remember that it is their imperative duty to maintain and improve the body corporate.

Science and Medicine.

Sir James Paget and Dr. Lauder Brunton.

The reproach of empiricism is being removed from medicine at a rate which would have startled the medical mind of only twenty years ago. It is true, indeed, that no amount of scientific knowledge can replace clinical experience, but it is to the combination of the two that we must look if we are in search of the ideal practitioner. Every scientific man will welcome the tone of the inaugural address delivered by Sir James Paget before the Abernethian Society, at St. Bartholomew's Hospital, on October 11. That a man cannot be at once a scientific man and a good practitioner was characterised by Sir James as sheer nonsense, and he proceeded to show in how many ways the science of medicine could be advanced, even by a busy man in general practice, if he were possessed of a truly scientific mind. He emphasised also the importance of the collection of facts, verified again and again till they were absolutely certain. In all that he said we most heartily concur. Few applied sciences are so complex as that of medicine, and in none is there need of more close and accurate observation. This power is to be attained only by scientific training, and it is to the superior education in science now afforded to the medical student that much of our recent advance is due, while still more may be looked for in the future. No better maxim can be taken as a guide in science than the sentence of John Hunter's quoted by Sir James Paget : "Do not think : try: be patient : be accurate."

If further commentary on Sir James Paget's address be needed, it may be found in Dr. Lauder Brunton's Harveian Oration, delivered a few days later before the Royal College of Physicians. The one man above all others from whose discoveries modern medicine may be said to date is undoubtedly Harvey: and Harvey was a practical physician no less than a brilliant investigator. This year's Harveian Orator is himself an example of the same combination, and in bestowing the Moxon medal for clinical research upon Sir William Jenner, the College of Physicians has shown its recognition of the high value to be set on the results of scientific method in the hands of a busy practical man.

There is, however, another side of the matter. Every practitioner may be and should be a scientific man, but it is not by the labours of the practitioner alone that modern medicine has advanced. Of this fact Dr. Brunton's Harveian Oration affords many instances He devoted the major part of his time to a consideration of the

modern developments of Harvey's work, and here it is to be observed that the pure physiologist is to the front. The specialisation demanded by modern science renders this imperative, and all that can be asked of the practitioner is that he shall keep himself abreast of the discoveries of the physiologist and learn to apply them in the treatment of disease. It is the intelligent application of the discoveries of pure science which forms the field in which clinical medicine and surgery have made and will make their most rapid advances, and it is in this field that a scientific training and habit of mind will be of most value to the practitioner.

Antitoxins.

The latest application of pure science to practical medicine consists in so-called "serum therapeutics." Although diphtheria antitoxin is still on trial as a means of practical treatment, there can be no doubt that its discovery is an immense advance in medicine. Chemical theories of immunity now bid fair to oust from the field the theories based on the doctrine of phagocytosis. "Für böse Gifte, Gegengift," sings Brangäve, in 'Tristan und Isolde,' and it is a good motto for the modern pathologist. There is already strong evidence that the 'Gegengift,' manufactured in the animal body as the result of a non-fatal attack of tetanus or diphtheria, can be used with effect in the treatment of the same diseases in man; but some time must elapse before good statistical evidence is forthcoming as to the effects of the diphtheria antitoxin. And for this reason. It is a usual custom in this country to regard as diphtheria all cases of membranous sore throat, nor can it be doubted that it is a wise practice to treat them as such. But statistics are in this way largely invalidated. Conclusive proof of the diphtheritic nature of a sore throat can be obtained only by the recognition of the characteristic diphtheria bacillus, and it is, perhaps, chiefly on this account that the foreign statistics of diphtheria mortality appear so much higher than ours, since only cases in which this evidence is present are admitted into the category of true diphtheria. When this source of fallacy is removed in this country by a systematic bacteriological examination of all cases of suspected diphtheria, we shall be in a position to estimate at its true value this latest development of science in the treatment of a hitherto intractable disease.

An Electrical Theory of Vision.

An interesting communication, brought before the British Association, and as yet unnoticed in our columns, was Professor O. J. Lodge's suggested electrical theory of vision. This was preceded and introduced by experiments illustrating Clerk Maxwell's theory of light. The necessary electrical radiations were obtained from a sphere charged from an induction coil; for their detection an arrangement of loosely-packed particles was employed. These, under the influence of the surgings of the electrical radiations, attracted one

another, and so became more closely packed. When closely packed they allowed a current to pass through more readily. The arrangement is termed a "coherer," on account of the capacity of the constituent particles to become more closely packed. After coherence the original looseness and consequent resistance may be restored by tapping. Professor Lodge's "coherer" consisted of iron borings loosely placed in a glass tube. The glass tube was placed in a large copper "hat" to protect it from electrical influences other than the radiations. The radiator was cased so that its radiations were directed to the coherer, and the coherer was read by a reflecting galvanometer with which it was placed in circuit.

With these appliances there were shown :—The influence of the radiations on the conducting power of the coherer; the polarisation of the radiations by gratings of parallel copper wires, the vibrations in the direction of the length of the wires being arrested, the others passing on; the reflection of the radiations from bright metal surfaces, from wood, and from the human body; the transparency of glass to these radiations, and the refraction of the radiations by means of a prism of paraffin: all indicating the analogy of these radiations to those of light.

From such experiments came Professor Lodge's theory of vision, which, however, he put forward as the suggestion of a theory rather than its promulgation. Briefly put, his suggestion is that light may excite transverse vibrations in the rods and cones of the retina, that these vibrations may alter the resistance of some part of the eye and so cause the recording of the light stimulus, and that the stimulus corresponding to the process of tapping for restoring the coherer to its original state would be the influence of darkness. Between the stimulus of light and that of darkness there would be persistence of vision. These ideas were illustrated by means of the coherer, the restorative tapping being effected automatically through vanes revolving by clockwork.

In the subsequent discussion, Lord Rayleigh insisted on the necessity of the three-fold nature of colour-vision being taken into account in any theory of vision; Professors Armstrong and Fitzgerald naturally laid stress upon the physico-chemical side of vision and the resemblance of the retina to a sensitised plate; while Professors Rutherford and Schaefer discussed the anatomical structure of the eye as a coherer. But Professor Burdon Sanderson made what, in our view, was at once the most obvious and the most valuable criticism, when he pointed out that, while Professor Lodge's suggestion was worthy of consideration as a theory of the action of light upon the retina, it was not a theory of vision at all.

Mind and Body.

The point raised by Dr. Burdon Sanderson in connection with Professor Lodge's suggested theory of vision raises the perennial difficulty

of mind and body. In the question of vision this difficulty is strikingly obvious. We can see that light has a direct effect upon the protoplasm of many of the simplest animals and plants. Many single-celled forms of life, imprisoned in a vessel, darkened, for instance, by being covered with black paper except at one spot, will crowd towards the window of light; others will shun the light and assemble in the darkest regions. In many of them, again, when they are observed under the microscope by a beam of light reflected through them by the mirror of the microscope, the direct effect of light as a stimulus may be seen by their sudden movements when the mirror is flashed on them or turned off. From conditions so simple up to the complicated eye of the mammalia an infinite number of grades of complication in the arrangement of cells, specialised in the direction of sensitiveness to light, may be observed, while, again, there are similar series of grades of elaboration of accessory structures. Thus there are structures that seem to be mechanisms directly optical, like convex transparent lenses and corneas to focus the beams; curtains of pigment, perhaps corresponding to the velvet sheet of the photographer; pigments that are directly affected by the chemical action of the light, like the sensitised film of a photographic plate.

Some of these arrangements are fairly well understood; about the function and mechanism of others theories are numerous and divergent. But were they all understood, not vision, but the mechanism of vision, would be understood. The appreciation by the brain or by the mind of the sensational stimuli afforded it by the optical apparatus involves many things that are not optical. It involves memories of previous visions, memories of touch, memories of movements, and several other factors. The analysis of what we know as sight, and the correlation of the component elements with the results of chemical, physical, and physiological investigation of the eye, is one of the obscurest tasks of the psychologist.

Realism in Art.

As an obvious example of the difficulties attending the phenomena of vision, consider the dispute in art connected with what is called realism. We may dismiss, first of all, idealised compositions, pictures in which the imagination of the artist frankly transforms his impressions of Nature. Take three types of paintings or drawings, each of which are set forward by some and accepted by many as adequate representations of what may actually be seen. To the first of the three types belong the landscapes of Mr. Waller Paton, scenic arrangements like Mr. Frith's celebrated "Derby Day," or the later works of Sir John Millais. Round such as these one may notice a group of admirers crying, "See, it is Nature itself! Look at the sheep on the distant hill with their owner's initial in red paint on their fleeces, while on the grass-blades in the foreground behold here

a beetle, there a caterpillar crawling." To the second type belong the striking productions of the modern impressionists; to many these appear chaotic daubs; an increasing number of people believe that the impressionists alone have the secret of producing by their canvases the same impressions that Nature produces. The third type, which is that most astonishing to the Western mind, consists of the strange Art of China and Japan, an Art so incongruous with our ideals that, at the first seeing, we hold it a fantastic perversion of the real. Yet these yellow artists would say "lying prone on the ground we draw things as we see them, and the lines and marks of our patient brushes show our brothers what we have seen."

Leaving aside all questions of art and technique, it is plain that the same things appear in different ways to different men, and to different races of men. Some of these differences may depend upon difference in structure of the optical apparatus, and so may have a physical explanation; many of them seem more likely to depend upon differences in mind or brain, and are matters for the psychologist.

Physiological Apparatus for Plants.

Workers in plant-physiology will find a description of several new and useful pieces of apparatus in a recent number of the "Minnesota Botanical Studies" (Bulletin no. 9, part iv.), a publication of the Geological and Natural History Survey of Minnesota. The first, devised by A. P. Anderson, is a balance with a self-registering mechanism for recording the rate of transpiration and its periodicity (if any) during a certain time. It consists of a balance, one arm of the beam of which is lowered by the increase in weight of a calcium chloride absorber. The lowering of the arm closes a circuit, and an electro-magnetic mechanism releases a weight which falls into the scale-pan attached to the other arm. The scale is thus balanced automatically after an increase equal to the weight used has taken place. At the instant the weight is released it is recorded on the registering cylinder of a recorder. The whole is enclosed in a case, for protection from falling moisture.

A new electrical auxanometer and a continuous recorder are described by W. D. Frost. The former was devised for such delicate measurements as of growth in thickness of stems and fruits, but is equally efficient in measuring growth in length, while its extreme lightness and delicacy make it especially useful in measuring the growth of small plants. The frame-work of it is made of aluminium, and the whole instrument weighs fifteen grams. For measuring growth in thickness it may be attached to the plant, increase in size of which unwinds a thread from a pulley, while the turning of the latter, by means of a ratchet-wheel, alternately opens and closes an electric current. The recorder and the auxanometer may be

separated any distance, and the former may also be used with other pieces of apparatus where a continuous record is desired.

The Discoverer of Dionæa.

In the *Journal of Botany* for November the editor gives some account of William Young, an American adventurer of the last century, who first brought to England, in 1768, living specimens of *Dionæa*, the carnivorous plant commonly known as Venus' fly-trap.

Young seems to have been a pushing, uneducated man, who, by dint of impudent self-assertion, got himself appointed Botanist to their Majesties, in 1764, much to the disgust and surprise of honest folk, like John Bartram and others, who knew him at home in Philadelphia. Apparently his only other claim to distinction rests in a MS. volume containing about three hundred rude figures of plants, and a corresponding volume of specimens, both now in the Botanical Department of the British Museum.

Change of Address.

We beg again to draw the attention of our readers to the notice that appears under the above heading on the last page of the present number. It is hoped that the new arrangement, which will come in force with the new year, will have the effect of facilitating our own business and our dealings with the public by concentrating the Editorial, Printing, and Publishing Offices at one address, instead of distributing them among three as has hitherto been the case. It was well at first to place ourselves in the kindly hands of so well-known a firm as Messrs. Macmillan, and we should like to take this opportunity of thanking them for the assistance and courtesy that they have extended to us. But now that we are growing up and begin to feel our legs, we are bold enough to think that we can walk alone. Whether we can succeed or not must of course depend not on ourselves but on the number of hands stretched out to help us. Since the Review will still be conducted by the same editorial staff and on the same lines as heretofore, we appeal confidently to our readers, subscribers, and contributors not to desert us; indeed, we venture to hope that they will now aid us all the more both in purse and person. If only their sympathy be assured, we shall step from the cradle with a light heart.

I.

The Homes and Migrations of the earliest known Forms of Animal Life, as indicated by Recent Researches.[1]

THERE is no portion of the geological record more important to the biologist than that which contains the history of the earliest known forms of animal life, for whether he believes in the theory of evolution or not, he is there made to see that there is a remarkable similarity in the forms which constitute the earliest faunas wherever they have been discovered. In speaking generally of these faunas we do not, of course, mean that similar marine animals occurred in each area at the same exact period in the world's history; on the contrary, if we admit that the animals migrated then as now when the conditions were favourable or otherwise, such would be impossible; but what is meant is that they occupy in the geological succession relatively the same general position. For instance, where the faunas, which we may here call A, B, and C, are found in conformable strata, the order of their appearance is the same in each area. If the sections have been correctly interpreted, C has in no case been found to precede A, nor B to succeed C. Year by year we have been made acquainted with fresh discoveries among the older rocks, but whether the evidence comes from Australia, China, India, Europe or America the fact remains that the order of the appearance of certain forms of animal life in the succession does not vary from that which had previously been made out in other areas. In the most favoured areas the rocks can only be expected to contain a very imperfect history of the animal life which existed at the time when they were deposited; still, sufficient evidence has been obtained to make it easy to realise what were the dominant forms prevailing at the time. For instance, it is well known that the earliest faunas are everywhere characterised by having Trilobites as their dominant organisms, and further that each fauna is distinguished by well-recognised genera with a comparatively limited vertical range. As the earliest faunas of which we have any knowledge contain well-developed forms belonging to widely separated groups, it is clear that we are here far from the beginning of life on the globe. How is it then that, with all

[1] Read at the meeting of the British Association, Oxford, 1894.

the careful researches which have been made of late years, no well-recognised forms have been discovered in earlier rocks than the Cambrian ?[2] Certainly, only a few sediments have yet been discovered of earlier date than the Cambrian which could have been deposited under conditions suitable to the existence of animal life; but some have been found, and yet up to the present they have only yielded very doubtful traces of organisms. The great physical changes which affected a very large surface of the globe before the Cambrian rocks were deposited possibly removed or obliterated much important evidence; but on the other hand it is more than probable that sediments containing many earlier faunas than those at present known to us are now submerged in the great oceanic basins.

It is generally admitted by geologists that the ocean basins have been great depressions of the earth's surface from a very early period in the world's history, but this does not of course mean that the same amount of land has been under water from that time to this. We know, on the contrary, by geological evidence that, owing to repeated physical changes, the area covered by water has varied much in extent at different times from the Cambrian period to the present; and it seems but reasonable to suppose that it did so also in Pre-Cambrian times. These changes, however, have been mainly along the borders of the oceans; hence the strata which have been brought to the surface are all comparatively shallow-water deposits. From what has been stated it would be anticipated that the earliest known faunas would be likely to have a fairly wide distribution over the globe, and that they would contain such forms only as could live near shores or in comparatively shallow water and were not easily affected by changes of temperature.

If the earliest known faunas lived along the borders of the ocean basins, it is but natural to suppose that pre-existing faunas, from which they were descended, inhabited similar localities; and we are justified in supposing that these basins when first tenanted by animal life were very much more limited in extent than they were at the commencement of Cambrian time. The lowest Cambrian rocks, being everywhere deposited near shore or in comparatively shallow water, tell us clearly that the depression at that time was still going on, and that the waters were spreading gradually over fresh land-areas. Most of the evidence obtained of late years bearing on these questions has been accumulated through researches in Europe and North America; therefore I propose to confine my further remarks to those portions of the globe.

As the surfaces of the Pre-Cambrian continents of Europe and America were very irregular when the lowest Cambrian deposits

[2] The paper by Professor W. K. Brooks, of which an account was given in the October number of NATURAL SCIENCE (vol. v., p. 242), deals with this question in a very interesting and nighly suggestive manner, and seems to me to explain some of the difficulties which meet the palæontologist in his inquiries.

were thrown down, it is only to be expected that the Cambrian rocks should vary considerably in importance in different areas, and therefore that the faunas also in some areas should be but poorly represented. Those parts which would be first encroached upon, if the depression was very gradual, as we have every reason to think it was, would contain the best record of the succession of life; and all recent researches have tended to show that the Cambrian faunas in the areas bordering the North Atlantic basin are the richest in the number of genera and species, and therefore contain the most perfect record of the succession of life in the early rocks hitherto obtained.

In a paper communicated to the Geological Society in 1875,[3] which referred mainly to the European areas, I stated that "the first fauna of which we have any knowledge occurs in the beds furthest to the west, and there in earlier beds than any which occur in the regions more to the east; and though the forms which make up this fauna belong to inferior classes, yet they are not the lowest types in those classes, but often show evidences of considerable progression in development. On this account I have often expressed the opinion that we were far from the beginning of this type of life even in the earliest Cambrian faunas, and that the forms had already undergone many changes previous to this period. It is easy now to see how these changes could have taken place, and moreover how it was that new forms so frequently appeared at certain stages highly developed and with no previous evidence in the rocks as to the changes they had undergone. The home of the earliest forms of life seems to have been somewhere towards the south-west, and possibly not far from the equator; and it is from here that the various forms seem to have migrated to the areas in which they were subsequently entombed. The migrations seem to have taken place towards the North-American continent very much about the same time as towards the European; and the sea-encroachments along that continent seem to have been in a direction from south-east to north-west, so that the lines indicating the two depressions would meet in mid-Atlantic. This accounts for the great similarity in the two faunas, and for the general resemblance offered by the order of succession of these early rocks in the two continents. The higher lands in America would be to the west and north-west, and the higher lands in Europe to the east and north-east; so that the last lands submerged would approach each other and occupy the same region of the globe."

When the above remarks were written, forms belonging to eight classes had been discovered by us in the Lower Cambrian (now called Lower and Middle Cambrian) in Wales; and in a paper in the *Quarterly Journal of the Geological Society* (vol. xxviii., p. 173, 1872) I stated that: "These same groups are also more or less present and tend to characterise these early deposits wherever

[3] *Quart. Journ. Geol. Soc.*, vol. xxxi., p. 552.

found ; but no country has up to the present time produced a more varied fauna or a greater richness in types than England. Scandinavia has a larger number of species, but not so many groups." The classes at that time represented in the Cambrian rocks of South Wales were the following, viz., Spongida, Echinodermata, Annelida, Brachiopoda, Gasteropoda, Pteropoda, Crustacea, Trilobita. In the table, in the paper above referred to, seven classes only are mentioned ; but since then it has been shown that the genus *Stenotheca*, which I had previously discovered in the Menevian rocks at St. David's, should be classed with the Gasteropoda.

Mr. C. D. Walcott, Director of the United States Geological Survey, in his very important memoirs, "The Fauna of the Lower Cambrian or Olenellus Zone,"[4] and "Correlation Papers, Cambrian,"[5] makes careful comparisons of the rocks on both sides of the Atlantic, which contain the Cambrian faunas, and says that "in lithologic, stratigraphic, and palæontologic characters, the Cambrian series on the opposite sides of the Atlantic are such that there is no hesitancy in considering them as belonging to one geologic group and as part of one geologic basin."[6] He further points out that the Lower Cambrian fauna, which he regards as a littoral fauna, is much more persistent in its character than that in the succeeding rocks, "for all the strata referred to the former horizon have been found to be characterised by essentially the same fauna." The Lower Cambrian fauna in America, according to Mr. Walcott, contains genera belonging to the following classes of invertebrate animals, viz., Spongida (4 genera), Hydrozoa (2 genera), Actinozoa (5 genera), Echinodermata (1 genus), Annelida (4 genera), Brachiopoda (10 genera), Lamellibranchiata (3 genera), Gasteropoda (6 genera), Pteropoda (4 genera), Crustacea (5 genera), Trilobita (15 genera).

Mr. Walcott's list contains a larger number of classes and of genera and species than is at present known in the Lower Cambrian fauna on this side of the Atlantic ; but, with the exception of the Lamellibranchiata, all the classes are now known to occur in the Middle Cambrian either of Wales or Scandinavia. It will be seen that Trilobita and Brachiopoda are the dominant classes, and this is equally marked in the succeeding or Middle Cambrian fauna. The Trilobita, especially, give to these faunas their very distinctive characters, and the faunas are frequently referred to as the *Olenellus*, *Paradoxides*, and *Olenus* Zones after closely-allied genera, which also are the most typical occurring in them. The genus *Olenellus* occurs only in the Lower Cambrian, *Paradoxides* in the Middle Cambrian, and *Olenus* in the Upper Cambrian. The order in which they appear in the succession is the same wherever they have been discovered in all the American and European sections.

[4] Tenth *Ann. Rep. U.S. Geol. Survey*, part i., pp. 509–763, pls. 49–98 : 1890.

[5] *Bull. U.S. Geol. Survey*, no. 81 : 1891.

[6] *Op. cit. ult.*, p. 372.

It is a tempting field for speculation to enquire why this should be the case. It has been suggested that *Olenellus* was a form peculiarly adapted for living in shallow water and along shore-lines; *Paradoxides*, on the other hand, is a much larger form, and would require somewhat deeper and more open water. It is the dominant form in the Middle Cambrian, and attained to a size of over twenty inches in length, being the largest trilobite known. When the Middle Cambrian rocks were deposited, a considerable depression had taken place, and though the depth was not great, the sediments must have been deposited in a fairly open sea. *Olenus*, the typical genus of the Upper Cambrian, was of small size, and has the appearance of a degraded form of *Paradoxides*.

When I ventured to suggest that the "home of the earliest form of life seems to have been somewhere towards the south-west, and possibly not far from the equator,"[7] I was, of course, referring specially to the North Atlantic Ocean. The other oceans would doubtless be equally tenanted by life, and would form centres of dispersion. In these early oceans there would, of necessity, be forms of life suitable to the varying depths; but it is not to be expected that representatives of all these could occur in any of the earliest faunas with which we are acquainted. When deeper water faunas are found in the succession, there, as in the littoral faunas of the early Cambrian, abundant appropriate organisms suddenly appear, showing that they had occupied areas favourable to their existence contemporaneously with the earlier faunas entombed in shallow-water deposits. As an instance of this we may mention the sudden incoming of the very rich graptolite fauna of the Arenig rocks.

HENRY HICKS.

[7] *Quart. Journ. Geol. Soc.*, vol. xxxi., p. 555.

II.

Cephalopod Beginnings.

THE chief object of this article is to draw attention to some observations recently made in North America by Mr. J. M. Clarke of Albany, and published in the *American Geologist* (6, 7, 8); to make in passing such criticisms as may seem desirable; and to consider the relation of the new facts to the general question of Cephalopod evolution.

First, however, for the sake of those whose studies have not lain specially among the Cephalopoda, it may be advisable to explain a few of the terms employed.

The shell of a typical cephalopod, such as *Nautilus*, *Ammonites*, or *Orthoceras*, consists of a very elongate cone, which may be straight or curved or coiled, but which is always divided interiorly by transverse partitions, or 'septa,' into a number of chambers, or 'loculi.' Broadly speaking, these loculi gradually increase in size from the apex of the cone towards its aperture, not merely because the width of the cone increases, but because the septa are placed at a gradually increasing distance from one another. The shell, as thus described, may be called the 'conch' or the 'phragmacone,' the latter word meaning nothing more than septate cone. The soft body of the animal is lodged in the wide end of the shell, in front of the last-formed septum; and the cup-like space in which it lies is called the 'body-chamber.' That part of the body which abuts on the last-formed septum and which fills the bottom or hinder part of the body-chamber, contains the chief viscera of the animal, especially the generative glands, and is known as the 'visceral hump.' This hump is covered with a thin, glandular skin, which secretes membranes of an organic substance called 'conchiolin'; as these membranes become impregnated with lime (calcified), the septa and the inner lining of the shell-wall are thus formed. The formation of septa in the cephalopod shell is precisely parallel to the formation of septa in other molluscs and in other groups of animals, such as the tabulate corals; that is to say, the soft body with its posterior secreting surface moves forward at intervals in its cone or tube. As to the causes of this forward movement there has been much speculation, into which it is now unnecessary to enter. One thing, however, seems fairly clear, namely, that the formation of

regular septa was a gradual development from a primitive non-septate condition.

A peculiarity that distinguishes the septate cephalopod shell from the septate shells or tubes of most of the other animals above alluded to, is the fact that an extension from the soft body always passes backwards through the septa and, however long the phragmacone may be, always appears to maintain connection with its apex. In such a form as the modern *Nautilus*, this extension comes off sharply from the visceral hump in the form of a thin tube, which passes immediately through the centre of the last-formed septum and stretches back through the loculi and through the centres of all the preceding septa. This fleshy tube is known as the 'siphuncle.' It will, however, be noticed that, just before the siphuncle becomes attached to the visceral hump, that is to say just where it passes through the last septum, it enlarges slightly, so that between the visceral hump and the siphuncle proper there is a small cone, which contains a slight extension of the generative viscera, and which may therefore be distinguished as the 'visceral cone.' Were all cephalopods fashioned like *Nautilus* in this respect, there would be no need to use this term 'visceral cone'; but there are many in which the visceral cone assumes very large proportions and appears to have given rise to some misunderstanding in the minds of those who have described such cephalopods. If we define the visceral cone as that part of the soft body which lies posteriorly to the inner margin of the last-formed septum, but anteriorly to the narrow siphuncle, we shall at once render futile many discussions that have taken place, as, for instance, those concerning the presence or absence of a siphuncle in *Sepia*, and we shall have a ready explanation for many puzzling structures in archaic cephalopods, such as the 'sheaths' and absurdly named 'endosiphon' of *Piloceras* and *Endoceras*. These, however, are details rather foreign to the present purpose. It is enough to point out that, whereas the siphuncle proper does not secrete a definite shell and merely becomes impregnated to a slight extent with carbonate of lime, the visceral cone, on the contrary, retains to a very large extent, in many cases, the typical secretive power of the visceral hump, by which shell-substance is eventually formed.

Having considered the phragmacone, or conical-chambered conch, in sufficient detail for the object of this paper, we turn now to a structure to which, under Owen's name of 'protoconch,' much attention has of late years been directed. Examination of the phragmacone of *Spirula*, of *Belemnites*, or of any ammonite or, better, of such a loosely coiled goniatite as *Mimoceras compressum*, will show that at the apex of the cone, which of course lies in the centre when the shell is coiled, there is a little round bulb, usually separated from the conch by a slight constriction (compare Figg. 1, *a* and 2, *b*). The siphuncle passes through the end of the conch into the protoconch; but instead of retaining its simple tubular character, it generally

enlarges within the protoconch into a rounded swelling, which is not attached to the wall of the protoconch. In *Nautilus* the apex of the conch is well known to be of a different nature. There is no rounded protoconch attached to the end of the cone, but the outer wall of the first loculus is marked by an elongate scar, the 'cicatrix,' which is at a little distance from the actual apex and on the inner side of the coil. Correlated with the absence of the globular protoconch, there is always a passage left between the inner side of the first loculus and the succeeding whorl of the shell. In other words, the umbilicus is perforate; whereas in the closely coiled ammonites this space is entirely filled by the protoconch. Round the cicatrix the shell is perceptibly thicker, and forms a raised margin to it. Within the first loculus, the siphuncle is tubular and cylindrical, and is attached to the wall of the conch almost, though not exactly, opposite the spot where the cicatrix lies externally.

When attention had once been directed to this distinction between the protoconch-bearing shells of the Ammonoidea and of the forms allied to *Belemnites* and *Spirula*, on the one hand, and the simple conchs of the Nautiloidea, on the other hand, it was supposed by many eminent writers, such as Barrande and Branco, that an insuperable distinction had been found between the Nautiloidea and the Ammonoidea; and Barrande especially, with his anti-evolutionist opinions, was delighted to prove, as he thought, that the ammonites could not possibly be derived from the nautiloids. So far was this distinction pushed by some, and notably by that ingenious thinker Munier-Chalmas, that the ammonites, goniatites, and their allies were bodily transferred from their position with the Tetrabranchiata of Owen's classification to the opposite division, namely the Dibranchiata.[1] Others, of whom Waagen (16), though with some hesitation, was the first, have adopted the simpler and the safer course of doing away with the classification of Owen, which, so far as it applied to fossils, was based on characters of which we knew and for ever shall know nothing, and of substituting therefor a division into three Orders, based chiefly on the structure and relations of the shell. These three orders are (i) the Nautiloidea, (ii) the Ammonoidea, (iii) an order equivalent to the Dibranchiata of Owen, for which I have proposed the name Coleoidea (1, 2), but for which others have adopted the (as it seems to me) less appropriate and rather misleading name of Belemnoidea (15, 4.)

It would be going too far afield to consider the very remarkable views as to the relations of the Octopoda that are held by Steinmann (15), or the modifications of Steinmann's classification recently proposed by Schwarz (13). The observations of Hyatt concern us more closely. This suggestive writer maintained some time since (11), on the evidence of specimens from St. Cassian preserved in the

[1] See for instance Bernard's "Eléments de Paléontologie," reviewed in Natural Science, vol. v., p. 298; October, 1894.

British Museum, that two species of *Orthoceras* (a genus universally ascribed to the Nautiloidea) did indeed possess very visible traces of a protoconch in the form of shrivelled masses of calcareous matter, which overlaid and obscured the cicatrix. The shrivelling was supposed by Hyatt to indicate that the protoconch in these forms was only slightly calcified, that, in short, it was chiefly formed of conchiolin. This would similarly account for its destruction in so many individual cases, and for its final complete disappearance in the subsequent history of the Nautiloidea. Upon this theory the gap assumed by Branco and Barrande to exist between Ammonoidea and Nautiloidea was to a certain extent bridged. "There is," wrote Hyatt, "convincing evidence in the structures of [certain] Cambrian shells that the Ammonoidea, with their distinct embryos, arose from the orthoceran stock, and passed through a series of forms, in times, perhaps, preceding the Cambrian, which were parallel to those characteristic of genetic series among Nautiloidea, viz., straight, arcuate, gyroceran, and nautilian." Six years later, in his magnificent monograph on the Genesis of the Arietidæ (12), Hyatt considered that the accumulated evidence had become yet more convincing; one observation of special importance was that the longitudinal striæ, which are so characteristic of many young nautiloids, were believed to pass from the conch itself onto the surface of the shrivelled mass which Hyatt claimed to be the protoconch.

Now, since so much stress has been laid, by Hyatt and others, on these observations, it behoves us to consider them with great attention. Fortunately the critical specimens are preserved in the British Museum, so that it has been possible for my colleague, Mr. G. C. Crick, and myself to subject them to a prolonged examination with the aid of the microscope. I am compelled, before proceeding with this article, to formulate our conclusions, which have reference to the figures on page 10 of "The Genesis of the Arietidæ":—

Figures 1–3. This specimen shows absolutely no trace of longitudinal striæ; therefore it is impossible for longitudinal striæ to pass onto the so-called protoconch, a statement based solely on the evidence of this specimen. On the other hand, it shows distinct traces of concentric striæ, which would doubtless have been more distinct had the specimen not been rolled and rubbed. There is no great distinction in colour or texture between the so-called protoconch and the rest of the shell. There are, however, clear signs that the shell has been flaked off from the end, and it seems perfectly possible that it once extended over the so-called protoconch. There is absolutely no evidence that the supposed shrivelled remains of the protoconch rest upon the cicatrix, or that they extend over the apical plate; on the contrary all the appearances suggest that this wrinkled mass passes up from inside the cicatrix, and that it is merely a plug, or infilling of secondary calcite deposited within the shell during the life of the animal.

Figures 4 and 5 are more correct, but the distinctness of the protoconch is certainly exaggerated. There is, indeed, a slight constriction, but there is no sign of shrivelling.

Figures 6, 7, and 8. This specimen shows no definite protoconch and hardly a trace of constriction. There is an abrasion of the surface, as shown in Fig. 7, but the shrivelling is greatly exaggerated. The outline of Fig. 6 is correct, and shows a slight swelling a little distance from the apex.

In short, we see no essential differences between these specimens and that represented in Figures 18 and 19, in which the protoconch is said by Hyatt to be removed and the opening to be plugged by secondary calcite, and which is actually figured for the sake of contrast with the specimens said to retain the protoconch. In making these criticisms, which are based on independent observation, we have the advantage of following the opinion of the great palæontologist, Barrande.

It does not follow from the preceding remarks that the species of *Orthoceras* in question (assuming that they do belong to that genus) were unprovided with protoconchs at some period of their lives; nor does it follow that the little lump seen at the ends of the specimens in the British Museum may not be some representative of a protoconch. It may very well be the calcareous infilling of a thin-walled protoconch; but there is no reason for regarding it as the shrivelled remains of a conchiolinous shell.

If therefore the general conclusions of Hyatt were to be accepted, it became desirable that some concrete instances should be found, on the one hand, of undoubted Ammonoidea that should be straight like an *Orthoceras*, and on the other hand of undoubted Nautiloidea that should possess a definite protoconch. When such a form was found, it would obviously be a matter of some difficulty to decide whether it belonged to the Ammonoid or Nautiloid branch; and if it really were the case that no criteria could be discovered, then such a form might claim to be a true connecting link. Still, there are certain minute features which are generally held, in a rough way, to distinguish these two orders; such are the position of the siphuncular passage through the septa, the nature of the external ornament, the contour of the septum, usually called the suture-line, and the proportions of length and breadth in the conch itself. These are characters well known to all who have ever worked on Cephalopoda, they are the ready means by which we tell an ammonite from a nautilus, and, as they are given with more or less accuracy in all text-books of palæontology, we need not further consider them. Enough to say that any straight, protoconch-bearing cephalopod shell can, by their means, be referred with a high degree of probability to its proper order.

The first step in advance was made by Branco, who showed that the straight *Bactrites*, from the Devonian rocks of Germany, possessed a well defined protoconch of ovoid shape (5). The affinities of *Bactrites* had before this been a subject of some dispute: while some placed it near the goniatites, others regarded it as merely a close ally of *Orthoceras*, differentiated therefrom only by the marginal position of the siphuncle. It was natural that the discovery of a protoconch

should, at least for a time, turn the scale in the direction of its ammonite affinities. To this argument Branco added the fact of its resemblance to the young of *Mimoceras compressum* and the presence of a dorsal lobe in the suture-line. This leads us straight to the recent observations of J. M. Clarke.

At various localities in Ontario and Livingstone counties, New York, at a horizon corresponding with that of the German *Bactrites*, Clarke has found numerous specimens, representing three or four species, of straight cephalopod shells, all possessing protoconchs. Some of these he refers to the genus *Bactrites* (7), while another he prefers to regard as *Orthoceras* (6).

Concerning *Bactrites*, Clarke is doubtful whether the specimens described by Branco were really of this genus; but he is convinced that his own material "corresponds, even to its specific characters, with that upon which the genus was established." Assuming the correctness of Branco's reference, the descriptions now given by Clarke supplement and in some respects set right those of Branco. The protoconch of the American specimens is not ovoid, but "bubble-shaped, frequently a little unsymmetrical or directed to one side, very broadly sessile upon the end of the shell tube, from which it is separated by a sharp constriction" (Fig. 1, *a*). The position of the siphuncular passage, even in the first septum, is distinctly lateral (Fig. 1, *b*). Where the siphuncle passes through the septa, it is surrounded by backwardly directed siphuncular collars or 'septal necks.' These septal necks are often so inclined as to be attached to the shell-wall, so that when the shell-wall is removed, the outer portion of the neck is almost invariably carried away with it. Thus an apparent dorsal lobe is formed (Fig. 1, *d*); and it is on this lobe, which does not really exist, that Branco has based some of his argument as to the affinities of his specimens.

The specimen referred by Clarke to *Orthoceras* is briefly described (Fig. 2, *a*, *b*). "It consists of the first or apical chamber of the shell, to which the protoconch is attached. The upper end of the specimen shows the first septum (not counting the 'apical plate' separating the protoconch from the first chamber) to be circular and with a central sipho. The lateral walls of the first chamber taper rapidly to the plane of conjunction with the protoconch and its depth is about one-half that of the latter. The protoconch itself is semi-ovoid in shape," and tapers slightly towards the apex as does that of *Belemnites*. It is of large size, as compared with the protoconchs of *Orthoceras* described by Hyatt, and "shows no indication of shrinkage or other irregularity, and its distal extremity is perfectly smooth." The specimen is referred to *Orthoceras* because the siphuncle (as shown in Fig. 2, *a*) passes through the centre of the first septum, and is not lateral as in the associated specimens of *Bactrites*. Larger specimens referred to *Orthoceras*, but without

the protoconch, do occur in these beds. The large size and unshrivelled condition of the protoconch is supposed by Clarke to be due to the fact that it may have been "derived from a shell so young that atrophy and wrinkling have not manifested themselves as they may have done with the more mature development of the shell."

The caution of this writer is so extreme that it is a little difficult to understand exactly what he does think, and it would be hazardous to give his conclusions in any words than his own. "If," he says, "the evidence presented brings us to the conclusion entertained by many of the older palæontologists, that *Bactrites* is closely related to *Orthoceras*, this conclusion is attained, by means of data not before elaborated, namely, the existence in both of like protoconchs. This fact, fortified by the decisive evidence that in *Bactrites* the sipho is strictly intra-marginal, the formation of a dorsal lobe wholly casual, leads to the conviction that *Bactrites* is little else than an orthoceran nautiloid with a lateral sipho. Both Branco and Hyatt have suggested the probability of *Belemnites* having been derived from orthoceran stock. Hyatt demonstrates that the guard or rostrum in this genus is a hypertrophic secondary deposition about the earlier parts of the true conch, similar to the plug which is sometimes found filling the distal fractured extremity of the sipho of orthoceratites. The similarity of structure in the true conch of *Belemnites*, *Bactrites*, and *Orthoceras*, is now increased by the demonstration of like protoconchs in all."

It is not clear whether Clarke would deny the goniatite affinities of *Bactrites*, or at least ally it more closely to the nautiloid *Orthoceras* than he would to any ammonoid; or whether he believes in the existence of a straight protoconch-bearing ancestor for each order, *Bactrites* being the ammonoid and *Orthoceras* the nautiloid ancestor, and the coleoid ancestor being as yet doubtful. The latter view, or something like it, seems to me to be nearer the truth; but I fear it is the former that is held by Clarke. It is therefore well to point out that his new observations do not necessarily support that view. If the protoconch which he figures in *Bactrites* does not so closely resemble that of *Mimoceras compressum* as the one figured by Branco, at all events the whole initial part of the shell, with its "*Oncoceras*- or *Gomphoceras*-like swelling of the shell-tube directly above the protoconch," bears a very striking resemblance to the young of *Agoniatites fecundus*.[2] Of course no stress can be laid upon the backward direction of the septal necks, for, as this is the direction in the ammonoid young, it is also the direction that we should expect to find in the ammonoid ancestor. The lateral, even if not marginal, position of the siphuncle is a bit of positive evidence strongly in favour of the ammonoid affinities of *Bactrites*. The sculpture also reminds one of the more primitive goniatites rather than of a nautiloid. The evidence of the so-

[2] Barrande. "Système Silurien," vol. ii., Suppl., pl. 490, fig. ii., 1.

called *Orthoceras* (Fig. 2) is also not wholly convincing. In this case the central position of the siphuncle is not positive evidence, for, as shown by Branco, Hyatt, and others, the siphuncle of Ammonoidea changes its position during growth.[3] It is certainly open to us to suppose that an ancestral form may have had a central siphuncle, and on *à priori* grounds one would expect such to have been the case. Consequently this so-called *Orthoceras* may not be a true *Orthoceras*, and may not be a nautiloid at all. If this were the case there would be no grounds on which to base any argument. But, assuming for the moment that Clarke is right in calling it *Orthoceras*, then it tends to prove precisely what we anticipated, namely, that the ancestral nautiloid had a protoconch; but it certainly does not prove that *Bactrites* was a nautiloid, any more than it proves that *Mimoceras* was a nautiloid, which latter would be a *reductio ad absurdum*.

We pass now to Clarke's last paper, which he somewhat rashly entitles "Nanno, a New Cephalopodan Type" (8).

The specimens described were found in the Trenton series at various localities in Minnesota. Their combined evidence enables us to describe the complete shell as follows. The general outline of the shell may be gathered from Fig. 3, *c*; it appears to be assumed that this represents almost the entire length of the shell, but the evidence that such is the case is not satisfactorily brought out, and one would imagine that the outer shell-wall might have extended further forward than is shown in this drawing. The phragmacone, then, consists of a relatively wide tube, suddenly swelling and again diminishing towards the apex. The septa are concave and relatively close together. They are penetrated by a relatively wide siphuncular passage, which has an absolutely marginal position just as in the Mesozoic belemnites. "The septa," says Clarke, "are abruptly deflected immediately about the sipho." This may mean either that they are bent upwards on that side towards the shell-aperture, or that their margins are bent downwards around the siphuncle so as to enclose it in a tube formed of septal necks. Either of these explanations is consistent with the figures, and the latter agrees with what we know of most of these primitive cephalopods; it is, in fact, this 'neck-tube' that is usually, though wrongly, termed the siphuncle by palæontologists. Wrongly, because the siphuncle, as already pointed out, is a fleshy extension of the soft body, and an extension which, strictly speaking, contains none of the viscera. In the present instance the width of the passage is so great that we may well suppose it to have been occupied by a visceral cone, rather than by a simple siphuncle; and this supposition would explain the apparent shallowness of the body-chamber in this form as also in *Piloceras* (see Fig. 5, I). In figures 3, *b* and *c*, the siphuncular tube is seen to be constricted where it passes through the septa, and this

[3] This also took place in *Orthoceras* itself, as lately shown by Foerste, but that does not affect the argument.

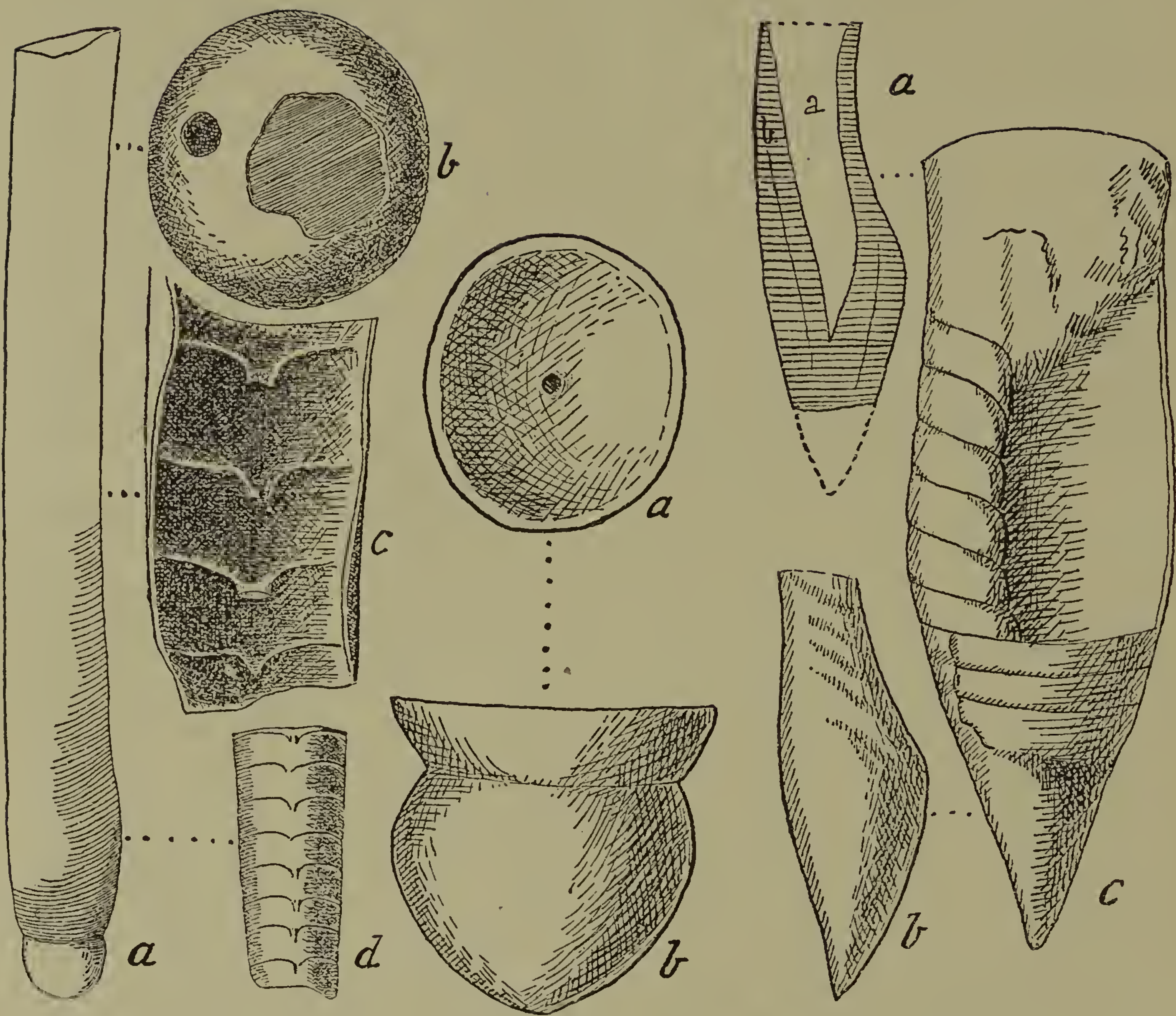


FIG. 1. FIG. 2. FIG. 3.

FIG. 1.—*Bactrites*. *a*. A shell retaining the protoconch and showing the expansion of the initial tube and the ornamentation (*B*. cf. *gracilis*). ×16. *b*. The distal surface of the first septum (somewhat broken), in *B. aciculum*, showing the lateral position of the siphuncular passage. The margin of the septum on the left is the line between light and shade. ×50. *c*. Interior of a portion of the shell of *B*. cf. *gracilis*, showing the intra-marginal position of the septal necks. ×4. *d*. Dorsal view of an internal calcareous cast of *B*. cf. *gracilis* showing the usual lobed appearance of the sutures. ×$\frac{3}{2}$.

FIG. 2.—*Orthoceras* (?), from Genesee shales. *a*. First septum, seen from above, showing central siphuncular passage. ×29. *b*. Protoconch and first loculus. ×35.

FIG. 3.—*Nanno aulema*. *a*. Vertical longitudinal section of the apical portion of the infilling of the siphuncular passage and protoconch; a. the internal cavity; b. calcite lining, with a streak indicating pause in deposition. *b*. A similar specimen seen from the exterior, showing swollen smooth apex and annulated siphuncular tube. *c*. The most complete example observed, showing siphuncular tube on left, constricted where it passed through the septa, lower down some septa, and then the conical apex. All ×1.3.

(All these figures are adapted from J. M. Clarke.)

also suggests that it was, during life, filled with some material that exerted pressure on its walls; the material would naturally be the generative glands.

The conical apex of the shell is not septate, but is entirely filled by the so-called sipho, which, before it passes through the first-formed septum, is greatly swollen so as to occupy all the space in the cone behind this septum, except a small wedge-shaped space next to the septum. This at least is what I gather from the following sentence, which I quote because I do not quite understand it. "The first septum seems not to conform to the contracted surface of the cone, which has a much greater obliquity, and there thus appears to be an irregular wedge-shaped cavity between these two surfaces, but there is no evidence whatsoever that the conical end of the sipho was in any way involved in this cavity except at its upper surface." This conical end of the siphuncular tube was covered by a very thin coating of shell, "a mere film." It has, however, been preserved to us, and preserved more constantly than any other part of the shell, owing to the fact that it became lined during the life of the animal by a secondary deposit of carbonate of line (Fig. 3, *a*). This deposit is very similar in structure and appearance to the well-known guard or rostrum of the belemnite; but, since it is inside and not outside the phragmacone, it cannot possibly be homologous therewith. It may better be compared to the plug of secondary calcite that repaired the ends of many individuals of *Orthoceras*, which were broken during life, or to the deposit that lines the siphuncular passage of *Actinoceras* (Fig. 6). Considerable stress is laid by Clarke on the fact that a break occurred during the deposition of this secondary calcite, so that it is divided into at least two portions "the line between which is represented only by a faint streak or difference in the texture." And this he regards as "indisputable evidence that the sipho enclosed or was composed of siphonal sheaths, as in *Piloceras*, *Vaginoceras*, and *Endoceras*." (See Fig. 5, I). This, nevertheless, I for one propose to dispute a little later on.

Before proceeding, however, with further criticism, it seems advisable to point out the very close resemblance that exists between these specimens now described by Clarke and specimens described nine years ago by Holm (10), which were found in beds of almost identical age in Oeland, Esthonia, and Prussia. A rough sketch (Fig. 4) made from some of Holm's figures by my friend A. H. Foord (9) is here reproduced, as it shows better than anything else could do the extraordinary likeness that obtains between the *Endoceras belemnitiforme* of Holm and the *Nanno aulema* of Clarke. In both we see the relatively close septa, penetrated by a wide siphuncular passage on the extreme margin (Fig. 4, *c*); the constriction of this passage by the septa, which bend downwards as septal necks and curve upwards towards the shell aperture on the side of the siphuncle; the swelling of the passage so as to fill almost, but not quite all the apex of the

cone; the thin shell-wall, and the lining of the apex by secondary calcite. The only differences to be detected from the figures are the cigar-like swelling of the slightly smaller phragmacone in *Nanno*, and the straighter external edge of the siphuncular passage in *Endoceras belemnitiforme*. These, however, are features of specific value at the very most, and it certainly seems that Clarke describes his specimens as representing "a new cephalopodan type," only because, in a state of oblivion most unusual with him, he has overlooked not merely Holm's elaborate paper but also Foord's well-known Catalogue of Fossil Cephalopoda in the British Museum. Whether Holm, who discussed the affinities of his species, will now be inclined to accept its generic independence must be left for him to say. But before these new

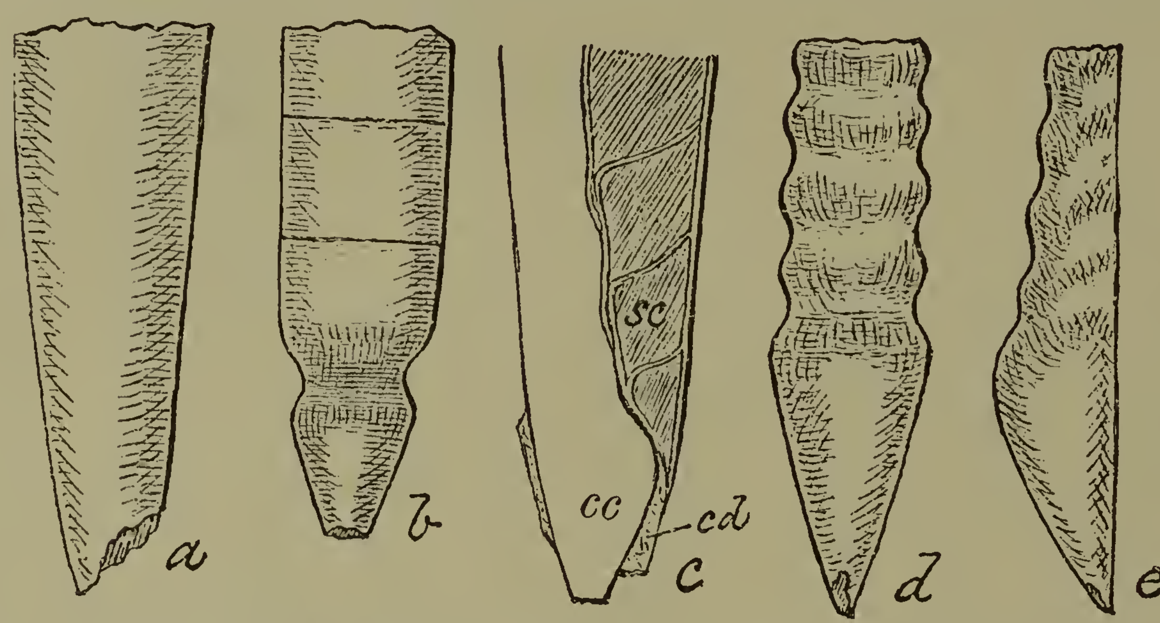


FIG. 4.—*Endoceras belemnitiforme*, Holm. *a*. Apical portion of phragmacone, covered with test. *b*. Internal cast of the same, denuded of the test, showing two septa. *c*. Sagittal section of the same, showing conical chamber (*cc*) at the apex of the siphuncular passage, some of the loculi (*sc*) separated from the siphuncular passage by the septal necks, and the secondary calcareous deposit (*cd*) lining the apex. *d*. Internal cast of the siphuncular passage and protoconch. *e*. Side view of same; compare Fig. 3 *b*. All × .6.
(Drawing by A. H. Foord, after G. Holm; the block kindly lent by Dr. H. Woodward, F.R.S.)

names find general acceptance, one would be glad of some hint as to their meaning. *Aulema*, every schoolboy will tell you, means a piece of music for the flute; but *Nanno*—what is *Nanno*? As a Japanese would say, "*nanno*?, of what?"[4]

To return to the meaning and structure of these undoubtedly interesting forms. There is, I submit, no evidence that *Nanno*

[4] Since this was in type, Dr. Clarke, whom I had informed of my sanguinary intentions towards his offspring, has kindly written me on the subject. He says, "I do not at all agree with Holm in placing the shell within the limits of *Endoceras* . . . the true *Endoceras* is septate to the narrowest apical extremity which has been observed." Also, "*Nanno* is a reactionary euphemism. I had thought at first to call it *Featherstonhaughoceras*, after an early explorer of Minnesota, but my mind gave way, and so I wrote it *Nanno*, though she is only a dead nymph who was fond of a flute." Dr. Clarke has clearly a larger circle of female acquaintance than was possessed by the compilers of my various classical dictionaries; and for this the gods should undoubtedly still be praised.

possessed 'sheaths' like those of *Piloceras* and some species of *Endoceras*. The faint streak described by Clarke is nothing more than one may see any day in a common belemnite guard, and merely indicates a slight pause in the deposition of the calcite, of no morphological significance whatever. The sheaths of *Piloceras*, well shown in Fig. 5, I, are clearly defined structures of a totally different origin. It may be worth while considering what that origin is. It seems pretty clear that in most of these archaic Cephalopoda the visceral cone extended to the depth of at least two loculi down the siphuncular passage. Since, like the visceral hump, it had the power of secreting shell, it formed, when in this position, long septal necks which often fused to form the neck-tube. We know that in *Nautilus* and *Spirula*, after the secretion of the septal necks, the outer coat of the siphuncle, both inside and outside the region of the septal neck, becomes hardened by calcium carbonate; this gives it a certain rigidity, and assists its retention in the fossil state. The same thing must have occurred in the coat of the visceral cone. Now, in *Piloceras*, when the animal advanced in the shell its viscera naturally followed it, and by suction the walls of the visceral cone were drawn in so as to form the narrow and empty siphuncle. At least such would have been the case had not the stiffness of the outer coat prevented complete yielding of the skin, especially at the posterior part where the siphuncle tended to begin, but where the coat was most calcified. It must therefore have happened that the inner layers of the skin were gradually torn away from the outer layers. Another stiffening of the skin would take place higher up, and the process would be repeated. Thus the 'sheaths' of *Piloceras* are nothing more than the remains of these old coats, left hanging on the siphuncle all the way up the passage (Fig. 5, I, *r* and *s*). The siphuncle is of course the structure that is often perplexingly called the "endosiphon." In some species of *Endoceras* the secondary calcification of the skin of the visceral cone seems to have advanced further, and the siphuncular cavity is divided into chambers

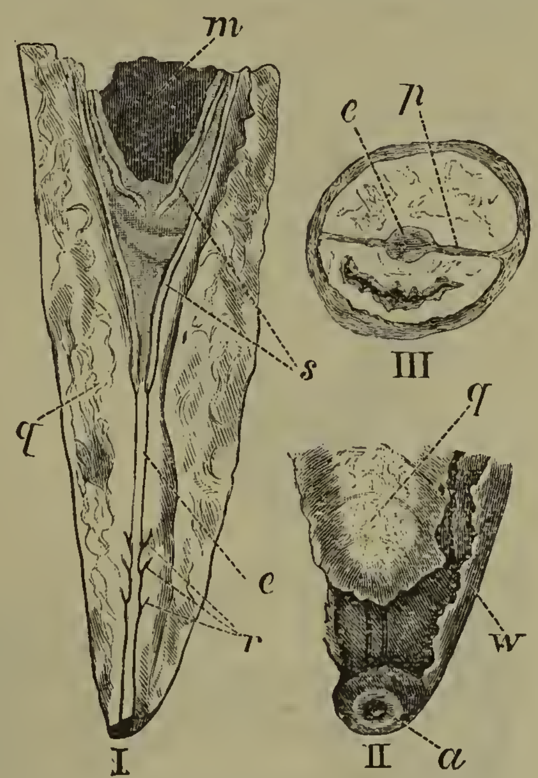


Fig. 5.—*Piloceras*. I. Longitudinal section of the siphuncular passage; *q*, quartz filling the greater part of the neck-tube cavity; *m*, matrix occupying the former position of the visceral hump; *s*, 'sheaths,' or calcified coats of the visceral hump; *e*, the siphuncle or so-called 'endosiphon'; *r*, remains of former sheaths (?). II. Posterior extremity of a similar specimen, much eroded, but showing an aperture, *a*, at the apex; *w*, the neck-tube; *q*, quartz infilling between neck-tube and one of the sheaths. III. Transverse section of a similar specimen, showing the outer wall or neck-tube and the siphuncle *e*; *p* is a partition the appearance of which is exaggerated and the significance of which is unknown.

(All figures of natural size; drawn by A. H. Foord; the block kindly lent by Dr. H. Woodward, F.R.S.)

by a series of hollow cones at irregular intervals. It is possible that the actual moment of casting off the slough was after the emission of the generative products, when the visceral cone was flaccid; this explanation coincides with Seeley's explanation of the origin of septation itself, but it is not exposed to the objections brought against the latter (14). There is, of course, no reason why "sheaths" should not have been formed in *Nanno*; but, to judge from the descriptions and figures of Holm and Clarke, no trace of them has yet been seen.

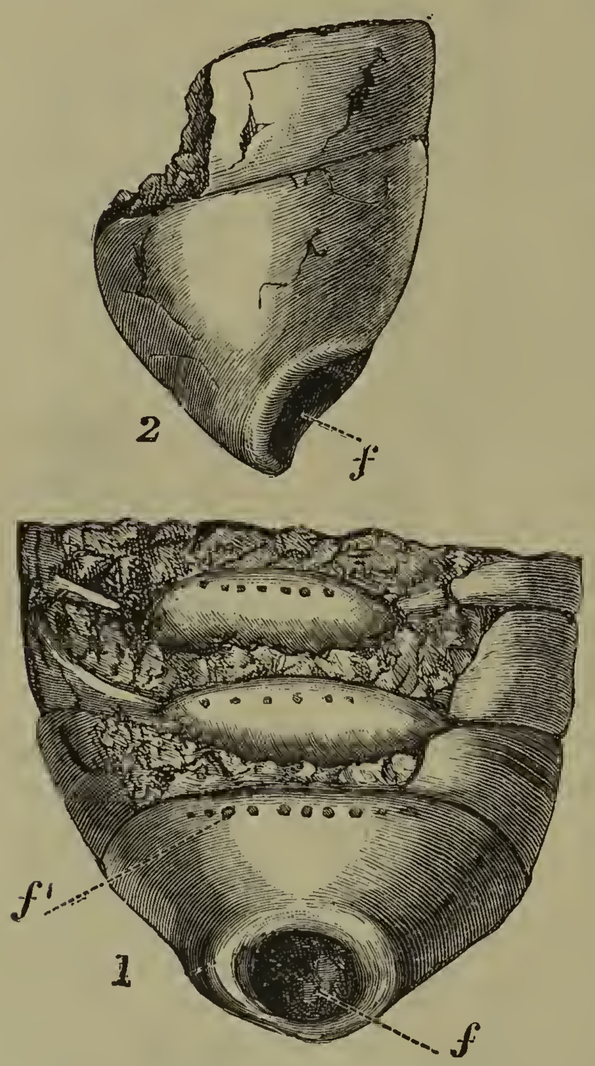


FIG. 6.—*Actinoceras*. 1. Front view of a weathered apical extremity, from which the shell has been removed. 2. Side view of another specimen. *f*, a large aperture at the apex; *f'*, the openings of small passages leading through the calcite infilling the neck-tube to the siphuncle, and probably representing the last relics of the attachment of the siphuncle to the wall of the neck-tube.

(Both figures natural size; drawn by A. H. Foord; the block kindly lent by Dr. H. Woodward, F.R.S.)

The feature of chief interest in *Nanno* is the apical swelling of the siphuncular passage. This was considered by Holm to represent the initial chamber, "Anfangskammer," by which he presumably meant the protoconch. Such, indeed, in spite of its large size, it appears to have been, and as Clarke observes, it probably contained the body of the young animal for a considerable period. That the forward movement of the young was quite gradual, is possibly indicated by the solidity of the calcareous deposit lining the walls of this apical chamber. If this suggestion be correct, it becomes interesting to compare this protoconch with the structures noticed above in the so-called *Orthoceras* from St. Cassian (see p. 425). The depression seen so clearly in Fig. 4, *b*, may well represent the constriction and the "shoulder" of the cicatrix alluded to by Hyatt in those specimens; while the plug of calcite in the *Orthoceras* is the diminished representative of the calcareous lining in *Nanno*. There is perhaps no importance to be attached to the fact that a swelling of the shell at a little distance from the apex occurs in *Nanno aulema*, *Orthoceras politum*, *Bactrites*, and *Agoniatites fecundus*; but it is certainly more worthy of remark than any resemblance to *Gomphoceras* and *Oncoceras*, such as Clarke insists upon.

Assuming that the somewhat similar structures seen in these forms are protoconchs in the strict sense, then it is clear that there is a great difference between them and such other primitive forms as

Piloceras and *Actinoceras*. As Clarke justly says of *Piloceras*, "were it possessed of such a solid apical sipho as is *Nanno*, that would be the part most readily preserved, as in this case." We have, however, more definite reason for believing that no such solid protoconch was present in *Piloceras* and *Actinoceras*. Figure 5, II., and figure 6 show at the end of the conch a wide aperture with a raised rounded edge. It is the opinion of Foord, with whom I heartily concur, that the siphuncle passed through this opening into the protoconch (9, pp. 159 and 166). The non-retention of the protoconch in a fossil state therefore seems due to the fact that it was composed of thin conchiolin, just as we have already inferred the protoconch of the ancestral Nautiloidea to have been; while the raised rim is remarkably like the rim of the cicatrix in a recent *Nautilus*. In these cases, not only was the protoconch more fragile, but the secondary infilling of the siphuncle tube did not extend into the protoconch as it did in *Nanno* and as it is here considered to have done in *Orthoceras elegans* and *O. politum*.

It appears a legitimate conclusion from the facts here quoted, that, however far back we trace the Cephalopoda, we still find two divisions: the one characterised by a relatively solid protoconch, often of relatively large size, and often preserved in a fossil state; and the other either possessing no protoconch, or one that was formed of material too delicate to be preserved, but characterised instead by an aperture with raised rim or by its degenerate representative, the cicatrix. To the former division are to be referred all the undoubted Ammonoidea and Coleoidea, while to the latter division are to be referred all undoubted Nautiloidea. We are therefore not entitled to say that the Ammonoidea were derived from the Nautiloidea, although we may not doubt that all three orders sprung from a common ancestral stock first evolved in far pre-Cambrian times. Of course it follows from this that the St. Cassian species referred to *Orthoceras* by Klipstein, as well as the specimen shown in Fig. 2, which Clarke refers to the same genus, cannot belong to the Nautiloidea at all, but are ancestral forms of the Ammonoidea and possibly of the Coleoidea. It is indeed probable that many other species hitherto referred to the loosely defined genus *Orthoceras*, or to some other genus of presumed Nautiloid affinities, will eventually have to find another resting-place. This is a question for those with more special knowledge than I possess.

Although in this article some adverse criticism has been bestowed on my Transatlantic friends, yet the facts that they have brought to our notice have a very real value. They have shown that the principles that have hitherto guided us have not been erroneous, and they have extended the basis of our argument by the width of a Hemisphere. I feel therefore some confidence in once more making a suggestion (3) originally placed before the Geologists' Association in March, 1888, but which, since it was not considered worthy of publi-

cation, did not find its way into print until quoted in the *Proceedings* of that Association in April, 1892 (4). The suggestion, put briefly, was that, at an early period, the Cephalopoda split into two great divisions: one division—which, starting with a very fragile protoconch, soon lost it altogether—may be called the *Lipo-protoconchia*, and is practically co-extensive with the Nautiloidea; the other division—which, starting with a stouter protoconch, preserved it, either by the coiling of the conch around it as in the Ammonoidea, or by its ensheathement in the mantle as in the Coleoidea—may be called the *Sosi-protoconchia*. And it was further suggested that such a classification, based upon ascertained facts of shell-structure, might prove superior to other classifications based upon suppositions as to the soft parts which could never be confirmed.

REFERENCES.

1. **Bather, F. A.**—"Shell-growth in Cephalopoda (Siphonopoda)." *Ann. Mag. Nat. Hist.* (6), vol. i., pp. 298–310; 1888.
2. ———— "Professor Blake and Shell-growth in Cephalopoda." *tom. cit.*, pp. 421–427; 1888.
3. ———— "Classification of the Cephalopoda." *Amer. Geol.*, vol. x. pp. 396, 397; 1892.
4. **Blake, J. F.**—"The Evolution and Classification of the Cephalopoda, &c." *Proc. Geol. Soc.*, vol. xii., pp. 275–295, with folding tables; 1892.
5. **Branco, W.**—"Ueber die Anfangskammer von *Bactrites*." *Zeitschr. deutsch. geol. Ges.*, vol. xxxvii., pp. 1–9; 1885.
6. **Clarke, J. M.**—"The Protoconch of *Orthoceras*." *Amer. Geol.*, vol. xii., pp. 112-115; 1893.
7. ———— "The Early Stages of *Bactrites*." *Amer. Geol.*, vol. xiv., pp. 37–43; 1894.
8. ———— "*Nanno*, a new Cephalopodan Type." *tom. cit.*, pp. 205–208; 1894.
9. **Foord, A. H.**—"Catalogue of the Fossil Cephalopoda in the British Museum, &c. Part I." London, 8vo; 1888.
10. **Holm, G.**—"Uebere die innere Organisation einiger silurischer Cephalopoden." *Palæont. Abhandl.*, vol. iii., Heft 1; 1885.
11. **Hyatt, A.**—"The Evolution of the Cephalopoda." *Science*, vol. iii., pp. 122–127 and 145–149; 1884.
12. ———— "Genesis of the Arietidæ." *Smithsonian Contrib.*, no. 673; 1889.
13. **Schwarz, E. H. L.**—"The Descent of the Octopoda, &c." *Journ. Marine Zool.*, vol. i., pp. 87–92; 1894.
14. **Seeley, H. G.**—"On the Septa and Siphuncles of Cephalopod Shells." *Quart. Journ. Sci.*, vol. i., pp. 760–762; 1864.
15. **Steinmann, G.**—"Vorläufige Mittheilung über die Organisation der Ammoniten." *Ber. naturf. Ges. Freiburg i. B.*, vol. iv., Heft 3, pp. 31–47; 1888. See also "Elemente der Paläontologie," 2 Hälfte, 8vo, Leipzig; 1890.
16. **Waagen, W.**—"Palæontologia Indica, ser. xiii., Salt-Range Fossils, 1b, Cephalopoda"; 1879–1880.

F. A. Bather.

III.

The Wing of Archæopteryx.

Part II.

I PROPOSE now to comment briefly upon a few of what seem to me to be the most important restorations hitherto made.

That of Professor (afterward Sir Richard) Owen first claims attention. This was the result of a careful study of the then recently-discovered fossil now in the British Museum. Bearing in mind the very fragmentary condition of the manus, this restoration must be admitted to be an exceedingly shrewd one. (See Figg. 1 and 2.)

We are told (14) that the "hand of Archæopteryx, besides being concerned in supporting the remiges or quill feathers of a wing, also supported two moderately long and slender free digits, each terminated by a strong, curved, and sharp-pointed claw. . . . The parts of the present skeleton show a certain amount of dislocation, and one of the claw-bearing digits may have belonged to the left [*i.e.*, right] wing." In the accompanying restoration, digit I. will accordingly be found to be marked with a ?, a doubt probably engendered by the obvious difficulty of accommodating the necessary carpal and metacarpal bones. We may, I think, safely predict that if Owen had had an opportunity of examining both the London and Berlin fossils at the same time, he would have given the London specimen a tridactyle manus, making his digit I. do duty for the penultimate and ungual phalanx of his digit III.; thus the wings of the two fossils would have been found to be precisely similar.

Of the Berlin example I wish to draw attention to at least six restorations. The first of these is that of Professor Vogt (20). In describing the manus he says: "The carpus shows only a single spherical bone." "On each manus (there are) three long slender digits, armed with claws" ". . . the radial digit or pollex (1) is the shortest, the other two are nearly of equal length, but the second is longest. These two digits were evidently united by tendinous and close aponeuroses; for in each manus these digits are placed in the same way, the one over-riding the other. The pollex is composed of a short metacarpal, a pretty long phalanx, and a terminal claw-bearing phalanx; the other two digits have, besides the metacarpal, three normal phalanges. The remiges were fixed to the ulnar side of the

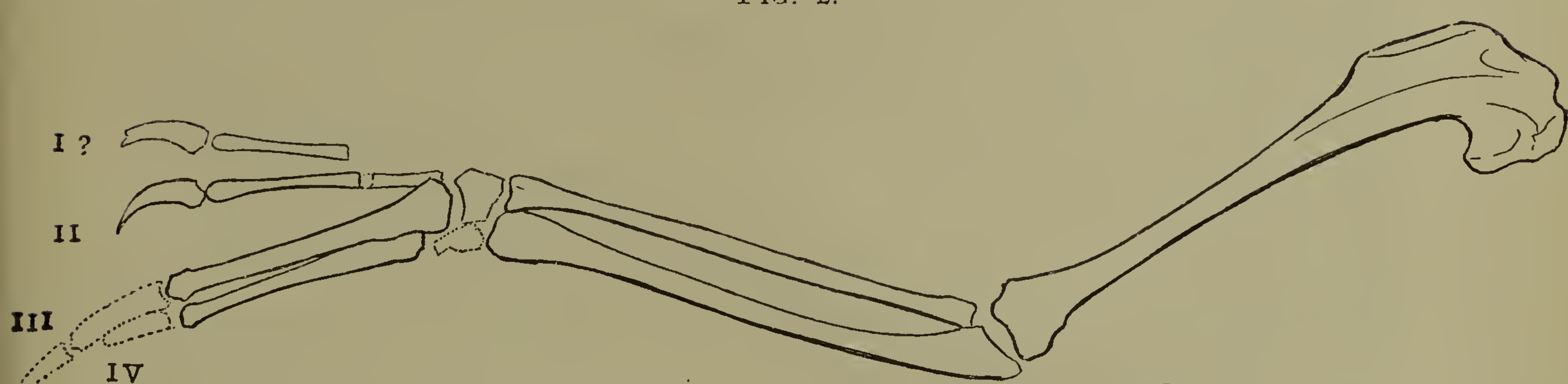
FIG. 1.—The specimen of *Archæopteryx* in the British Museum. About ½ natural size. Explanation of letters :—*1*, *2*, portions of wing-digits, with claws; *cr*, metacarpals; *u*, *u'*, ulna ; *r*, *r'*, radius ; *h*, *h'*, humerus; *sc*, scapula; *fu*, furculum; *c*, ribs partly sternal); *l*, feathers; *i*, innominate bone; *a*, acetabulum of the same; *f*, *f'*, femur; *t*, *t'*, tibia; *mt*, metatarsus ; *p*, phalanges ; *b*, supposed cast of brain.

The block kindly lent by Dr. H. Woodward, F.R.S.

forearm and manus, though no special adaptation to this end can be observed in the skeleton. The pollex . . . bore no bastard-wing. . . . The wing is rounded in outline, like that of a fowl." This description is supplemented by the accompanying figure, which, though somewhat diagrammatic, is correct in its proportions. (Fig. 3.)

Professor Dames in his exhaustive monograph (1) discusses the manus at considerable length. The carpus in particular receives a large share of attention. With Vogt, Dames agrees that only one free carpal bone is visible on the slab, and this, he says, from its position, must represent the radiale, and, judging from its size, probably afforded an articular face for the metacarpals I., II., and perhaps even III., though he suggests that it is probable that metacarpal III. may have articulated with a separate carpal—the ulnare, which is yet concealed under the stone. Attention is drawn to the large size of the radiale, Dames remarking that, while in adult living birds the radiale may vary in size relatively to the ulnare, in the embryo the radiale is the smaller bone, so that, coupled with the fact that there are no free distal carpals, the carpus of Archæopteryx agrees rather with the adult than with the embryo of living birds. The existence of two proximal carpals is considered almost certain, since he reminds his readers that, among modern birds, the only instances in which the carpus has been reduced to a single distinct bone[1] in the adult are instances in which the power of flight has been lost, *e.g.*, *Casuarius* and *Apteryx*.

Fig. 2.



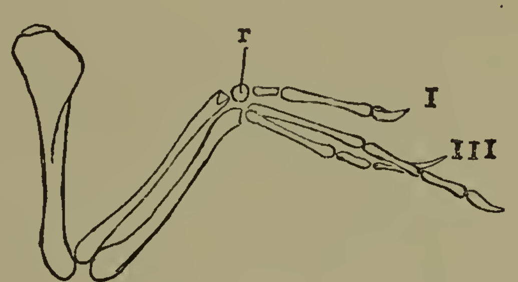


Fig. 3.

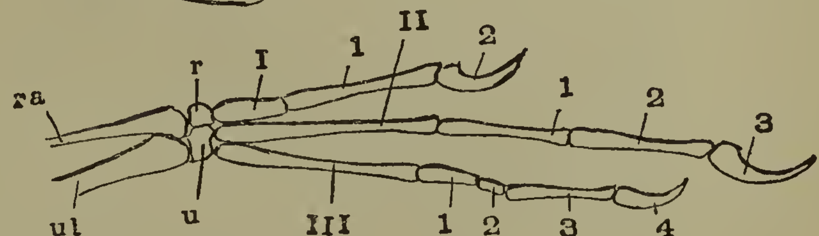


Fig. 4.

Restorations of the Wing of Archæopteryx.

Fig. 2. From the London specimen, after Owen.
Fig. 3. From the Berlin specimen, after Vogt.
Fig. 4. From the Berlin specimen, after Dames.

Further on Dames again refers to the carpus. Here attention is drawn to a groove running across the surface of the large free carpal

[1] The carpal in these cases is the ulnare.

(radiale) just described, which he suggests may—if it have any meaning at all—represent the boundary line between the radiale and ulnare, so that after all we may be dealing, not with one, but two bones, the homologues of the existing proximal carpals. As to the probability of a distal row of carpal bones, Dames writes: "There can be no doubt, speaking generally, that a distal row of carpals was laid down (angelegt), since they are laid down in every reptile, in every bird, indeed in every vertebrate, with the possible exception of the fish; and if this be admitted, the appearance of our specimen shows that they must have fused with the metacarpals." The nature of this view is next discussed. Quoting Rosenberg, he tells us that in the embryo of living birds the carpals 1–2 appear from the very first, not as separate elements, but as a single piece. Carpals 3–4 are similarly developed. The metacarpals[2] first approximate with their corresponding carpalia or their equivalents, and then fuse with them and with one another; but in Archæopteryx, we are reminded, these metacarpals are free: it is therefore open to question whether (i) the development of these carpals in Archæopteryx proceeded after this manner (*i.e.*, as carpals 1–2 and 3–4); or (ii) as carpals 1 and 2, 3 and 4; or (iii) whether the cartilaginous body, which Rosenberg supposed to represent 1 and 2, really only corresponds to 2, in which case the first metacarpal would articulate, not with a carpal of the distal, but of the proximal row—the radiale.

Now if my reader will turn to p. 353, Fig. I., he will see at a glance that Dames' interpretation of this region differs fundamentally from my own, inasmuch as what that writer believes to be the radiale I imagine to represent a fused row of distal carpalia, while the proximal carpals seem to me to be distinctly indicated, in the photographs, in the space between this distal row and the end of the forearm. On referring to the text, it will be seen that I have even gone so far as to suggest the presence of an intermedium. Now, supposing I am right, mark how much this matter will be simplified. Turning again to p. 353, it will be seen that in Fig. II.—the wing of the ostrich—we have precisely the arrangement of carpal bones that I believe to have existed in Archæopteryx, viz., two free proximal carpals—perhaps three—and a fused row of distal carpals. The difference between the two wings lies in the fact that in Archæopteryx the metacarpals were probably distinct from one another and from the distal carpal mass throughout life, whereas in the ostrich they become anchylosed when the adult stage is attained. Thus the carpus of Archæopteryx represents neither the early embryonic nor the adult stages of modern birds, but rather the late embryonic and post-embryonic stages. Although my view of the matter bears the stamp of probability, it cannot, of course, be accepted in any other light than that of a suggestion, and in this spirit

[2] Metacarpal IV. enjoys but a transitory existence.

I have submitted it to my critics, trusting that it may lead to a careful re-examination of the Berlin fossil, which, as I have previously stated, I have never seen.

Judging from Professor Seeley's description of this fossil (19), there seems a possibility that my rendering of the carpus may prove to be correct. He pointed out that it was a moot point whether the large free carpal described by Vogt belonged to the proximal or distal row, and in the same paper he says: "The middle metacarpal . . . so far as I can see, terminates proximally in a rounded carpal bone like that of a bird"; thus I imagine he believed, as I do, that the bone in question represented the distal carpals, and not the radiale, as Dames seems to think.

There seems to be some difference of opinion as to the number of phalanges in digit III., some describing three, others four. Judging from the evidence of the wing alone, one would be inclined to think that there were not more than three, and that the appearance of four phalanges was due to the fact that the first phalanx of digit III. had been snapped in two across its distal third, the dismembered portion afterwards slightly drifting apart. Since, however, we get precisely the same effect in both wings, this can hardly be deemed the result of an accidental fracture; though it is, perhaps, equally hard to account for the fact that they happen to have separated in an exactly similar way in each wing. Though this arrangement of a short phalanx intercalated with two long ones is not unique, it certainly in this case appears in its most exaggerated form.

Professor Dames has supplemented his description by an outline restoration, Fig. 4; this, however, is probably not intended to do more than represent in a very diagrammatic way the salient features of the wing. It is strange, however, that he should have based this restoration upon Gegenbaur's figures of the developing wing of an embryo chick, since, as Dames himself shows, this eminent anatomist was deceived as to the real nature of the avian embryo carpus. In Gegenbaur's figure referred to, there is but one row of free carpals, which represent the radiale and ulnare. The ulnare is of great size and not only supports digits II., III., but contributes to that of digit I. There is no indication of a distal row, for Gegenbaur believed that at the time of the differentiation of the cartilaginous skeleton, only the radiale and ulnare were present, and that, even in their beginnings they showed no trace of fusion with other carpal elements. In Dames' figure accordingly, the ulnare entirely supports digits II. and III., while the radiale bears digit I. Thus the ulnare is much larger than the radiale, whereas Dames laid great stress on the large size of the latter of these two bones in the fossil; so large, indeed, did it appear to him there, that he imagined it must certainly have afforded an articular surface for digits I., II., and perhaps even III. It is probable that, in studying Gegenbaur's paper, the groove across the "large free carpal," which he at first dismissed from con-

sideration as probably having no real significance, came back upon him with new force, this time bringing conviction with it.

The remiges or quill feathers are briefly referred to. Of the primaries (metacarpo-digitals), Professor Dames tells us that they were attached, not only to the metacarpus, but extended down to the claw of the longest finger—digit II. The shafts of these primaries, he considers, having regard to their function as "Schwungfeder" (quill feathers), to be somewhat thin and slender. Here, again, it will be seen that my restoration does not quite agree with Dames' description. Turning to Pl. II., Fig. I., it will be noticed that the quills are supposed not to have extended beyond digit III., and I think there is every reason to believe that this was actually the case. The shafts of these quills are certainly slender, but not relatively more so than is the case with very many birds of active flight to-day. In the large plate accompanying Dames' paper, all the remiges are represented as having a proximal overlap, an arrangement exactly the reverse of that which I have supposed to obtain when discussing this matter in Part I.

Our fourth restoration is that given by the late Professor Romanes[3] (17), Fig. 5. It is difficult to know what to say of this, for it agrees with neither the London nor the Berlin fossil, and looks rather like the outcome of a process of mental evolution than the result of a careful study of the actual specimens. In the figure it will be seen that though the relative length of the digits roughly corresponds to that of the Berlin fossil, the hand, as a whole, is too short. All three fingers are represented as perfectly distinct, freely moveable independently of one another, and apparently were not supposed to bear any part in the support of the remiges. It is just possible, however, that the third digit may have been credited with supporting, say, two or three primaries. Not even with a great stretch of imagination can the outline of the wing be likened to that of the Berlin fossil. I have taken it for granted that this restoration is intended to represent the Berlin example, and is not a combination of this and the London fossil, since in Owen's restoration, there were four digits, of which I. and II. were separate and apparently freely moveable, while III. and IV. (resembling digits II. and III. of modern birds) were supposed to have supported the primaries. Had Owen's restoration been present before the mind of the restorer, it is possible that these digits III. and IV., with their primaries, would have been made to supplement what were evidently meant for the three digits of the Berlin specimen. The animal is thus described (p. 172): "The extinct Archæopteryx . . . presents throughout its whole organisation a most interesting assemblage of 'generalised characters.' . . . For example, . . . its still

[3] This, it will be remarked, is stated to be "after Flower." Sir W. Flower tells me, however, that he cannot recollect either having published such a figure or having used it in lectures.

unreduced digits of the wings which (like those of the feet) are covered with scales." There is no *evidence* of the presence of scales on the feet, though they probably were thus clothed, but there seems

FIG. 5.—Restoration of *Archæopteryx*, $\frac{1}{3}$ nat. size, from "Darwin and After Darwin." The block has been kindly lent by Mrs. Romanes.

to me to be neither evidence nor *reason* to assume their presence on the fingers.

Dr. Hurst's restoration (6) took the world by surprise, but it remains to be seen whether it will stand the test of criticism. In order

that we may see the matter from Dr. Hurst's point of view, and thus enable ourselves to understand maybe the conclusion which he has arrived at, let us examine a few passages culled at random from his paper.

"The photograph accompanying this essay [reprinted on p. 351] shows the primary quills to have been supported by none of the first three digits, and justifies, if it does not even prove, the view that those quills were supported by the digits IV and V (or one of them). Portions of these large digits were figured by Owen thirty years ago, and they are seen in the London specimen and are quite unlike anything seen on the surface of the Berlin specimen, in which these digits probably still lie hidden."

Again: "If the dissected wing of a common bird . . . be compared with the plate of *Archæopteryx*, the conclusion that those two wings are essentially alike will be inevitable. It will be impossible to avoid the conclusion that the two digits which support the quills of the ordinary bird existed also in *Archæopteryx*, and their position will be seen to be indicated faintly in the photograph by a shadow which runs parallel with and behind the slender digits. The carpal angle of the wing will be seen in front of the carpal ends of the slender fingers, and from this point the outline of the anterior margin of the wing can be traced to the tip. This margin lies under those fingers. Not only did these, as their form and structure show, *not* support the quills, but they did not even contribute to the support of them. These fingers lie not in the wing at all, but *upon its feather-clad surface*. Those slender fingers, like the free fingers of the *Pterodactyla*, or of the recent 'flying' phalangers and squirrels, or of *Galeopithecus*, or like the pollex of a bat, are admirably adapted for climbing in trees. They proclaim *Archæopteryx* to have been a winged quadruped, . . ."

"Dames . . . states that the primary quills . . . were attached to the longest finger (II). If . . . we consider what would be the result of such an attachment, it must be obvious that it would be twofold. Firstly, the attachment of such a series of quills would render the fingers perfectly useless for climbing, and secondly, a single flap of that wing would twist the phalanges off at the joints."

Now as touching Owen's restoration. I have already pointed it out as more than likely that the phalanges Owen marked as digit I.? were really none other than the ungual and penultimate phalanges of digit II. Supposing this to be the case, it will be seen that the wing of both London and Berlin specimens are precisely similar.

We may therefore assume that the remiges in both fossils would have been attached in the same manner. If it can be proved then, so far as proof is possible, that these digits *could* have supported the remiges, we shall have gone a great way towards demolishing, or at any rate rendering pointless, Hurst's elaborate hypothesis. I feel convinced

that I have already shown this to be more than probable, and it remains for my readers to decide whether I am right, or whether, with Dr. Hurst, we are to regard these digits as too "*weak-jointed*" to bear the "torsional stress" to which they would be subjected.

As to the slenderness of the digits, I have already pointed out that relatively to the size of the wing they are not more slender than is the case in living birds; further, in so far as the metacarpals are concerned, that of digit III. is stouter than obtains to-day. The wing of the young ostrich presents an interesting exception (see p. 353, Fig. II). Here metacarpal III. is a relatively stout bone, as in Archæopteryx, but differs therefrom in being more bowed. Indeed this wing seems to represent a stage in the evolution of the avian manus between those of Archæopteryx and modern Carinatæ, since it possesses a fused mass of distal metacarpals, approximated to, but not fused with the three free metacarpals, and since all its digits end in a claw. Another stage is afforded by those birds that have the third metacarpal reduced to a slender bar, and only digits I. and II. ending in a claw; while in the highest birds even the claws are lost. As both Professor Fürbringer and Dr. Gadow have already shown, it is very evident that the ancestral ostrich must have possessed the power of active flight, and this power must therefore have been acquired while the metacarpals were yet free, as in Archæopteryx. Thus, then, we have a chain of connecting links between the wing of this ancient fossil and those of the highest living birds.

The weak-jointedness of the phalanges hardly need detain us long, inasmuch as, so far as I can see, the phalanges of living birds seem to be scarcely if at all more firmly bound together than were those of Archæopteryx; indeed, remembering the strong tendinous fibres and bands which bind together both skeleton and the supported remiges, any special mechanism or anchylosis of the joints would seem to be unnecessary. The presence of the digits which supported the primaries, Dr. Hurst tells us, is indicated on the slab by a "shadow which runs parallel with and behind the slender digits." Among some notes on the subject taken in Berlin, which Dr. Hurst has generously furnished me with, I find the following reference to these digits and the shadow:—"Supposed position of IV. and V. wrong! Supposed shadow is a yellow stain—? iron, and the supposed IV. and V. if present are completely hidden." This last qualification, "if present," looks very like a doubt and but for the fact that in a letter lately to hand he says "I do not in the least think that what you brought forward at Oxford weakens my case," I should have believed that he had surrendered his position, and therefore have considerably curtailed my review of his paper.

Yet a few more words remain to be said as to these additional fingers. Supposing them to exist, where did they articulate? The whole of the carpal space is evidently monopolised by the "three free digits," so that there seems no room for two, or even one more

finger, especially since that digit (or digits) must have been larger and stronger than any of the three now exposed.

A very serious objection, to my mind, against the probability of Dr. Hurst's theory is the obvious fact that the manus of Archæopteryx must have been highly specialised in two directions to fit it for two distinct modes of locomotion—flight and climbing—modifications, too, which must have gone on hand in hand, a possibility which I can

a) bats
Pterodactyl.
Galeopith:
Petaurus.

FIG. 6.—Reconstruction of *Archæopteryx*, from Koken, "Die Vorwelt"; by kind permission of the publishers.

hardly imagine. That the free fingers *have* become specialised, either for the purposes of climbing or those of flight, there can be no doubt, since no one would regard these digits as representing even a part of the unspecialised primitive manus adapted for ordinary walking or climbing purposes. In conclusion, I perfectly agree with Dr. Hurst when he says: "If Natural Selection has been operating long enough

and efficiently enough to determine the evolution of so perfect a series of feathers, it is perfectly certain that the same selection will have led also to the evolution of supports for those feathers as fully fitted to support the feathers as the feathers are fitted for flight; and even if there had been no indication of those supports on the slab, we need have still had no doubt as to their existence."

Professor A. Andreæ has recently distributed among his friends an outline restoration of Archæopteryx, which I have had an opportunity of seeing. Since it is not published technically, I am debarred from meting out to it the criticism that seems appropriate. Let it suffice to draw attention to the fact that the metacarpals are too short relatively to the length of the forearm, and that digit II. is too short relatively to the length of the metacarpals, while digit III. is too long by the same standard of measurement. This comparison, of course, is made with the Berlin fossil, from which these digits have been restored. The additional digits demanded by Hurst for supporting the quills have also been added, for the sketch was, as I am told, intended as a graphic interpretation of Hurst's paper.

Yet another restoration is that of Koken (10). This is a very interesting one, and is evidently founded upon Dames' hypothesis that the quill feathers "arose, not only from the metacarpus, but also from the phalanges of the middle of the three fingers, *i.e.*, II. It can be seen especially clearly on the left wing, if the quill of the first feather be prolonged towards the hand, that it must have rested upon the penultimate phalanx of the middle finger, close behind the claw." I think, however, that if Professor Dames will examine this fossil again, he will find that he ought to substitute the third for the middle finger as the recipient of the first primary.

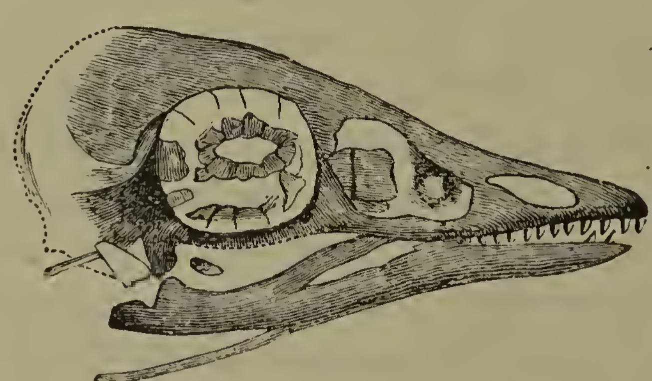
According to Koken the wing of Archæopteryx was still relatively small, and as Figure 6 shows, he believes the III. digit not to have been concerned in the support of the remiges, but to have been entirely free and functional. These remiges, it will be seen, have evidently been drawn without reference to their precise form and length as seen on the slab.

But for fear of becoming wearisome to my readers, I would fain pass on to review briefly a few of the more striking theories as to the manner in which flight has been acquired, having, of course, especial reference to Archæopteryx. I feel, however, that it would be wiser to leave this matter for a future paper. This course is the more advisable, since it is hardly possible to enter upon a discussion of this nature without taking into consideration the whole skeleton. Already I have found it difficult at times to confine myself to the narrow limits of the title of the paper. In such a discussion the reptilian affinities of Archæopteryx, real and imaginary, would necessarily take a large share, and this would mean straying from the point at issue as well as doubling the length of the article itself.

REFERENCES.

1. **Dames, W.**—"Ueber Archæopteryx." *Palæont. Abhandl.*, Bd. ii., Heft 3; 1884.
2. **Dollo, L.**—"L'Archæopteryx." *Revue quest. Scient.*; 1885.
3. **Evans, J.**—"Portions of a Cranium and a Jaw." *Nat. Hist. Review*, vol. v., p. 415; 1865. A reprint issued in 1881, p. 5.
4. **Fuerbringer, M.**—"Über das Schulter-und-Ellenbogengelenk bei Vogeln und Reptilien." *Morph. Jahrb.*, Bd. ii., p. 118; 1885.
5. **Gadow, H.**—"Bronn's Klassen und Ordnungen des Thierreichs." Anatomischer Theil, pp. 75, 503, 566; 1891. Systematischer Theil, p. 88; 1893.
6. **Hurst, C. H.**—"Biological Theories, VII. The Digits in a Bird's Wing." Natural Science, vol. iii., p. 275; 1893.
7. **Huxley, T. H.**—"Remarks upon *Archæopteryx lithographica*." *Proc. Roy. Soc.*, xvi., p. 243; 1868.
8. ————————"American Addresses." Lecture II., pp. 57–69; 1876.
9. ————————"Anatomy of Vertebrated Animals." P. 291; 1871.
10. **Koken, E.**—"Die Vorwelt." P. 369; 1893.
11. **Leighton, V.**—"The Development of the Wing of *Sterna wilsoni*." *Amer. Nat.*, vol. xxviii., p. 161; 1894.
12. **Marsh, O. C.**—"Jurassic Birds and their Allies." *Amer. Journ. Sci.*, ser. 6, vol. xxi., p. 318; 1881.
13. **Morse, E. S.**—"Tarsus and Carpus of Birds." *Ann. Lyceum Nat. Hist. New York*, vol. x., p. 141; 1874.
14. **Owen R.**—"On the Archæopteryx of Von Meyer, &c." *Phil. Trans.*, vol. cliii., p. 33; 1863.
15. **Parker, W. N.**—"Elements Comp. Anat. Vertebrates." P. 106; 1886.
16. **Pycraft, W. P.**—"Wing of Archæopteryx." *Journ. Oxford University Junior Scientific Club*, no. 12, p. 172; 1894.
17. **Romanes, G. J.**—"Darwin and After Darwin." 1892.
18. **Rosenberg, H.**—"Ueber die Entwicklung des Extremitäten-Skelets." *Zeitschr. wiss. Zool.*, vol. xxiii., p. 142; 1873.
19. **Seeley, H. G.**—"On Carl Vogt on Archæopteryx." *Geol. Mag.*, Dec. 2, vol. viii., pp. 300; 1881.
20. **Vogt, C.**—"*Archæopteryx macrura*." *Revue Scientifique*, ser. 2, tom. 9, p. 241 1879.
21. **Woodward, H.**—"On a Feathered Fossil from the Lithographic Limestone of Solenhofen." *Intellectual Observer*, Dec., 1862, pp. 317 and 319.
22. **Zittel, K. A.**—"Handbuch der Palæontologie." i. Abth. Palæozoologie, Bd. iii., Lief. 4; p. 821; 1890.
23. ————————"Archæopteryx ein Zwischenglied zwischen den Vogeln und Reptilien." *Der Naturforscher*, no. 42, p. 401; 1879.

Oxford. W. P. Pycraft.



Archæopteryx.

From a photograph of the specimen now in the Berlin Museum, $\frac{2}{7}$ natural size. Reprinted from "Lectures on the Darwinian Theory," by A. Milnes Marshall, with the kind permission of Dr. C. F. Marshall, at whose special request the figure is printed in the reverse position to that in which it is usually presented.

IV.

Pseudo-Biology.

OF all the pseudo-sciences that flutter abroad in this *fin de siècle* and attempt, often with too much success, to inveigle the unwary, none is so popular, none so fatally attractive, as that whose professors preach from the pseudo-biological pulpit. It is because it deals so largely with an assumed biology in its bearings on the conduct and progress of our selves, our nation, and our race, that it attracts us first, and that if we follow its will-o'-the-wisp, we founder in ignominious error. Its vogue is also great because so many of us poor mortals are wandering around in the darkness, having deserted the hand that formerly guided us and still searching for another; then to us wanderers there appear evangelists of a new gospel, which is superficially intelligible, and which we readily accept because we really understand it no more than do its preachers.

There has been lying on our table for some months, now looked at and now cast aside, a book by that extraordinarily popular Professor of this subject, Mr. Henry Drummond. It has been on our minds, at intervals, to give some review of this book, but we have never been able to decide whether we should be warranted in devoting any of our space to its consideration. And we only do so now in response to the request of numerous correspondents, whose minds seem, not unnaturally, to have been disturbed by the diverse criticisms that this work has called forth.

The title of the work in question is "The Lowell Lectures on the Ascent of Man." The Lowell Lectures are, we believe, delivered in Boston, one of the chief intellectual centres in the United States; and we doubt not that the audience attending them would be worthy of any lecturer in the world. Remembering this, we have, often and often as we perused Mr. Drummond's pages, wondered with a mirthful and a sorrowful wonder to think of our cultured Bostonian friends and the keen youth of Harvard sitting in solemn rows, agape and aghast at the meretricious and fallacious periods of this pseudo-biological pulpiteer. What did the cute sophomores from Cambridge make of this marvellous utterance?—"Run the eye for a moment up the scale of animal life. At the bottom are the first animals, the Protozoa. The Coelenterates follow, then in mixed array, the Echinoderms, Worms, and Molluscs. Above these come

the Pisces, then the Amphibia, then the Reptilia, then the Aves, then—What? The Mammalia, THE MOTHERS. There the series stops. Nature has never made anything since . . . the one motive of organic Nature was to make Mothers. Ask the Zoologist what Nature aspired to from the first, he could but answer Mammalia—Mothers." And so on. Surely, like the old maid in the story, those young men from Harvard shouted back to the Professor, "SPEAK FOR YOURSELF, SIR!"

But, after all, there are many worthy people, especially among the purchasers of Christmas booklets, who adore Mr. Drummond's eloquence, so we will not mar the goodwill of the season by over-insistence on our own squeamishness. Only, if the odious comparison be for a moment allowed, we would venture a word of praise for the sober and lucid style of the professor's rival in this field, Mr. Benjamin Kidd. A book may be written well or ill; its solid scientific or, if you prefer it, theological merit is not thereby affected. It is when rhetoric usurps the place of reason and leads to an irreverent disregard of truth that we have a right to complain of its abuse. And it cannot be denied that there are in this book many mistakes, perhaps of no great importance, but which, when found in the writings of a professor, it is kinder to ascribe to rhetoric than to ignorance. Here is a curious instance (p. 14), "The man who is busy with the stars will never come across Natural Selection, yet surely must he allow for Natural Selection in his construction of the world as a whole. *He who works among star-fish will encounter little of Mental Evolution*, yet will he not deny that it exists." We should think not, indeed. Has Mr. Drummond ever heard of G. J. Romanes, whose work "Jelly-fish, Star-fish and Sea Urchins" was distinctly subservient to his work "Mental Evolution"? Here is another case, more patent and less excusable (p. 240), "The Birds took one road, the Vertebrates another; the Vertebrates kept to the ground, the Birds took to the air." There are, it is true, people who still insist that "an animal" must have four legs and a tail; but surely even a Professor of Pseudo-biology might be expected to put the Birds among the Vertebrates. Here again are two statements, either of which is questionable, but which, occurring in adjacent chapters, also involve a contradiction: on page 245 it is written, "The first law of Evolution is simply the first law of motion. 'Every body continues in a state of rest, etc.'"; but on page 309 it is written, "The first commandment of Evolution is 'Thou shalt mass, segregate, combine, grow large.'" We hope that Mr. Drummond is obeying the former rather than the latter, we had liefer see him in a state of rest than growing large.

Mr. Drummond will complain that we are trifling with him and not arguing. That is so; but then Mr. Drummond should not trifle with us. The lapses to which we have referred will mislead no sensible person; it is merely inexcusable that a sensible person should

be subject to them. But there are other erroneous statements, which often form the bases of arguments, and which would naturally deceive the lay reader. The following sentence (p. 22) is an important link in one of Mr. Drummond's main chains of argument, viz., that proving Altruism to be the "direct outcome and essential accompaniment of the reproductive process." The sentence is, "Without some rudimentary maternal solicitude for the egg in the humblest forms of life, or for the young among higher forms, the living world would not only suffer, but would cease." There are sea-urchins that carry their young in a little pouch on their backs, there are fish that make nests, there is a toad whose back is pitted with cells in which the little toads develop; but where is the maternal solicitude in the common *Echinus*, in the fishes of the sea, or in the frogs of the marsh? No one would deny maternal care to be a great aid in evolution, serving as it does to transmit the experience of a lifetime; but it is stretching a good many points to say that "for a time in the life-history of every higher animal the direct, personal, gratuitous, unrewarded help of another creature is a condition of existence." One begins to suspect that this professor of pseudo-biology has never seen so much as a tadpole. Then again, in the chapter on the evolution of language, we are treated to this remarkable pronouncement, "Any means by which information is conveyed from one mind to another is Language. And Language existed on the earth from the day that animals began to live together. The mere fact that animals cling to one another, live together, move about together, proves that they communicate." So doubtful is this that one might even take so highly developed an animal as the Rosy Featherstar of our seas, and ask Mr. Drummond what real proof he has that the individuals of its floating colonies communicate with one another.

The preceding instances should show the admirers of this latter-day pamphlet-writer that they ought to be careful in accepting his premises as gospel-truth. And if they are not yet convinced, let them ask some biological friend what he thinks of the following observations. "It is one of the most astounding facts of modern science that the first embryonic abodes of moss and fern and pine, of shark and crab and coral polyp, of lizard, leopard, monkey, and Man are so exactly similar that the highest powers of mind and microscope fail to trace the smallest distinction between them" (p. 79). Astounding indeed! Small wonder that in the description of the development of the human embryo which immediately follows, Professor Drummond definitely introduces the formation of a gastrula by simple invagination. This prepares us for the description of the simplest case of cell-division, on p. 282, "At one moment it will call in matter from without, and assimilate it to itself; at another moment . . . it will set a portion of that matter apart, add to it, and finally give it away to form another life. Even at its dawn life is receiver and giver; even in protoplasm is Self-ism and Other-ism." The only difficulty is

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that, let Professor Drummond study his divided cells for ever so long, he will never be able to tell us which is the Self and which is the Other—

For each of them is born a twin,
And not a soul knows which.

Or what will the biologist say when, to impress upon us his idea that all sociability depends on Sex, Professor Drummond writes, "There is no instance in Nature of Division of Labour being brought to such extreme specialisation" as in the evolution of Sex? He will remind us of the colonies of jelly-fish, in which some individuals do nothing but swim, others nothing but eat, others nothing but produce their kind, while one serves as a swollen bladder or float to support the whole colony; or of the coral-like colonies, where some individuals are mouths and some are only fingers to feed the mouths; or of the sea-mats, in which some of the polyps in a colony are nothing but tweezers to hold on to the food, while the other individuals eat it; or if colonial animals be put out of court, he will adduce the ants, with their castes of soldiers, workers and the rest, and the bees, with workers and drones and a single queen. All this specialisation has nothing to do with Sex, and yet in many cases Mr. Drummond's very words are applicable; each individual has "so entirely lost the power of performing the whole function that, even with so great a thing at stake as the continuance of the species, *one* could not discharge it."

This last quotation and the criticism of it that any biologist might be expected to offer, lead us directly to Mr. Drummond's main argument, "the fundamental omission," as he calls it, from all previous theories of the world, and also show us how greatly Mr. Drummond has overstated his case. We are told in the first place that all except a few powerful intellects, such as "Le Conte, Mr. Herbert Spencer, Romanes, Miss Buckley, and Prince Kropotkin," have taken the Struggle for Life to be the all-important factor in the Evolution of living beings. Drummond (let us drop the unnecessary "Mr.") insists that the Struggle for the Life of Others is equally important. The Struggle for Life is connected with the function of Nutrition, with the struggle for food, which alone has been considered heretofore. The Struggle for the Life of Others depends absolutely on the function of Reproduction. This great discovery, which is to "readjust the accents," must have occurred to Drummond very shortly after reading a book, not altogether unknown, entitled "The Evolution of Sex," and written by those original thinkers and writers, Patrick Geddes and J. Arthur Thomson. To insinuate, or even to suppose, that our leading writers on these subjects have overlooked the principle of altruism throughout the animal kingdom, evidences a remarkable state of mind. Everyone has long recognised it, and everyone would be prepared to grant to the various actions of animals grouped around reproduction, whether the attraction of the male to the female, or the tending of the offspring by the mother, a large share in the fostering of an altruistic

spirit. But Drummond rides a willing horse too hard. He tries to make his phrase cover many things to which it is not applicable. What does he mean by the "Struggle for the Life of Others"? On page 39 it appears to mean nothing more than the struggle to produce, the tendency to grow fat and multiply. But this is not an obvious meaning of the phrase. The fact is that, in ascribing all animal acts to the two functions of nutrition and reproduction, Drummond overlooks the inner impulse of life that pervades all living beings, the joy of life, so wonderfully described by Mr. Hudson in "The Naturalist in La Plata," the Over-Soul as Emerson calls it, the excess of vitality that is the very spur to sexual love and the essence of all reproduction. There is no "Otherism" here; it is the intensest of "Selfism." There is hardly a passion in all animal life so purely selfish as this one, which Drummond places at the base of the Struggle for the Life of Others. Moreover, this Over-Soul it is that produces over-population, which is the great goad of progress. But this is the very thing that makes the Struggle for Life keen, and he is either very bold or very foolish who would try to capture this for the side of Altruism.

Surely the truth is, what we thought everyone recognised long since, that the altruistic principle springs chiefly out of the gregarious habit, which has not necessarily anything to do with reproduction. Regarded in this light the Struggle for the Life of Others is merely a development of the Struggle for Life itself. But here is the point which so many of these pseudo-biologists seem to miss. The individual is not the unit of evolution. The unit is the Species, and, as Mr. Kidd has so well pointed out, the interests of the species are directly opposed to the apparent and immediate interests of the individual. In the lower animals, where reason does not come into play, there is nothing to run counter to the forces that make for the progress of the species, and these have produced the subservience of individuals to the colony or tribe in those cases above referred to. Even in the human species, we should in many cases be inclined to reverse Drummond's idea (p. vii.) that Family life precedes the Tribe.

It is when we turn to rational man, that we see difficulties in the way of further altruistic developments. Man is an egoist as well as a gregarious and political animal. He is an individualist by reason, if not by nature. Mr. Kidd's point appears then to be this. Religion is neither true nor false, natural nor supernatural,—merely the apotheosis of the principle that governs the life of all herds, families, tribes, nations, viz., self-sacrifice and the subordination of the individual. The apotheosis of this altruistic principle gives it what Mr. Kidd calls a "sanction," which, so far as the individual is concerned, is "ultra-rational." The philosopher understands the drift of altruism, he sees its origin and its necessity. Not so the hungry man in the street or the forest. He needs something outside to keep him in the path of allegiance to the race. This is religion,

the "something not ourselves that makes for righteousness," which, whether false or true, as we now choose to understand falsehood and truth, has been the motive force in the rise of many nations and of vast empires. This is the force, strictly subordinate to the great principle of Natural Selection, and subject like all else in this world to the process of Evolution, this is the force that has worked side by side with the simple struggle for the life of the individual, to produce the world as we see it to-day, with its men and its women, its fathers and its mothers, its citizens and its patriots.

But Drummond understands neither this, nor Mr. Kidd, nor even Professor Huxley. Wresting from its meaning a sentence by the latter, he tells us (p. 4) that "Evolution is simply 'history' . . . neither more nor less than the story of creation." This is disingenuous: Evolution is more than this; it implies, as Huxley proceeds to say, "a series of genetic changes," "progressive differentiation," and "the growth and modification of primordial germs." "Evolution," says Drummond again on p. 20, "began with protoplasm and ended with Man." Who is this person, so ignorant of Laplace and Kant, of Lockyer and Crookes? Who is this presumptuous prophet that sets a limit to the thoughts of God? After this we do not wonder that every thesis of this chatterer should be vitiated by his idea of his own self-importance. *He* knows the end of Creation, and so he tells us that, "the beginning must be interpreted from the end": the cart, with Professor Drummond holding the reins, must be put before the horse. We prefer to wait; the end is not yet.

With its cheap philosophy and its reading into the lower world all the passions and ideas of our own highly-organised consciousness, this book is a sad example of teleological argument. Teleology it is that causes Drummond to speak of "the gradual tempering of the Struggle for Life. Its slow amelioration, &c." The nature of the struggle changes, it passes to a higher plane; but the fight is not less keen, the pain not less bitter. Let this Professor in his chair contrast the social wreckage of our race, the tramp, the prostitute, the starving seamstress, with the jelly-fish stranded on the beach or the larvæ swallowed at a gulp by the monsters of the deep, and then let him dare repeat to a despairing world, that the harsher qualities of the struggle are passing away!

Our author is pledged by his preface to be original, and this is how he effects his purpose. "One has only to read the average book of science," he writes on p. 42, "to wonder at the wealth of knowledge, the brilliancy of observation and the barrenness of idea. On the other hand, though scientific experts will not think themselves, there is always a multitude of onlookers waiting to do it for them. Among these, what strikes one is the ignorance of fact and the audacity of the idea." "The Ascent of Man," then, is original because it falls under neither of these censures; for it combines "ignorance of fact" with "barrenness of idea." F. A. Bather.

SOME NEW BOOKS.

The Naturalist in India.

A Naturalist on the Prowl, or in the Jungle. By Eha. 8vo. Pp. xii., 257. London: Thacker & Co., 1894. Price 8s. 6d.

"For their first introduction to the public," writes the author in his preface, "these papers are indebted to the *Times of India*." The editor of that newspaper is clearly a discerning person, and the author certainly cannot be mistaken in thinking that he "detected on the kind countenance of the public" much approbation of the articles—at least that is our opinion after reading this charming volume. "Eha," whoever he may be, has clearly some leisure which he has well spent in communing with nature in India. Nature in India is much more of a "rum'un" than she is at home here. She has produced, for instance, a serpent, the King Cobra; this *Ophiophagus*—whose bad example of eating other snakes has been recently followed at the Zoo by the Boa, proving the adage that evil communications corrupt good manners—is one of the few wild beasts that will deliberately go out of its way to attack a man. The power of the human eye may be a delusion, but there are not many wild animals that take the initiative in declaring war upon our species. The particular Hamadryad upon which nearly a chapter of the book is written was discovered coiled up upon a tree, and was at first asserted by the natives to be a Python. The reason for this varnishing of the truth was that the native preferred that "Eha" should dislodge the brute. A glance at the face of the reptile at once dispelled the python theory. "A python's eyes look nowhere in particular," we are told, "this creature's eye met mine with a truculent stare like nothing I had seen before." Some more information about the Hamadryad is given which runs as follows: "On the way I learned some things about the King Cobra which are not generally known. So swift is it that, when it pursues, escape by flight is impossible. When it has caught a man it swallows him whole, then climbing a tree, it winds itself round the trunk and tightens its coils until the man is crushed all to nothing in its inside. Thus it digests him. Facts like these are becoming increasingly rare in books. You must glean them among the simple folk who spend their lives with the beasts of the field and from infancy hold converse with nature's charms and view her stores unrolled. I have gathered many such very wonderful and known to few." It is not often, we must add in bare justice, that "Eha" is so newly-humorous as in the above passage.

A chapter entitled "The Voice of Mirth" gives the author the opportunity of saying something about the Cicada. The present writer has himself heard the "protracted ear-rending scream" of this insect, which is far worse than that of any other insect known to

science. Here it is the male who indulges in the shrill music. "Happy is the Cicada," as the ancient and misogynistic philosopher remarked, "in that he has a voiceless wife." "Cherchez la femme" is the explanation that the author, unlike others, is not altogether disposed to adopt in explanation of the sounds. He thinks that the vibration caused to the body by the noise is probably pleasing to the Cicada himself, and that he sings purely for his own amusement. It has been remarked that in the east there is no object in the sluggard fatiguing himself by going to the ant; the ant comes to him in some force. Chapter xvii. commences with one of Mr. Sterndale's excellent and humorous sketches of the author in the very act of grappling with ants upon his neck and ants upon his leg. The red ant of the east is one of the drawbacks to residing there. A study of the ways of this particular species is not conducive to the reception of moral lessons of any great value. They wait for you, "dancing with excitement on the point of every prominent leaf," as you take your walks abroad; the older members of the community have even, according to "Eha," an accurate anatomical knowledge of the human body, and at once make for such points as are more especially tender. Everybody knows that in this country there are ants which foster Aphides in order to feed upon the sweet secretion produced by them. The little black milkmaids throng round the little green cows, tap them gently with the antennæ, and drink up the milk which they then furnish. The red ants in India keep a butterfly caterpillar of more than an inch long for the same purpose. In gratitude for past favours, the chrysalis is carefully watched over, and the resulting butterfly allowed to fly away in perfect safety.

This is a sample of much new and readable information.

Indian Geology.

A Manual of the Geology of India. Chiefly compiled from the Observations of the Geological Survey. Stratigraphical and Structural Geology. Second Edition. Revised and largely re-written by R. D. Oldham.[1] Calcutta, 1893 (received 1894). 8vo. Pp. xxiii. and 543, with folding map, 23 plates and 27 figures.

Blanford & Medlicott's "Manual of the Geology of India" has long been an ideal book of its kind. Detailed descriptions of the geology of extra-European countries have been issued in a separate form far too infrequently. The geologist, and especially the palæontologist, who has occasion to refer to some point in the stratigraphy of such a country has, as a rule, to search through numerous scattered memoirs for the simple fact he needs. Moreover, probably most geologists enjoy the occasional perusal of the description of some country if written as a connected story, and not in a series of scattered memoirs, dealing with local details. The publication, therefore, of a general account of the geology of India was an act for which all geologists must have felt grateful to the authorities of the Geological Survey of that country. The first two volumes, which contain the general stratigraphy, were issued in 1879, and have ever since served as indispens-

[1] We understand that a new title-page has been issued, owing to the omission of the names of the two original authors, Messrs. Blanford and Medlicott. For this, however, we understand Mr. Oldham was not responsible. He had inserted the names of the two original authors. He also enables one to see at a glance how much credit is due to Messrs. Blanford and Medlicott by printing, in the list of contents, the titles of all paragraphs taken from the former edition in different type to that used for those for which he has contributed.

able books of reference, and as models of what this sort of work should be. The fact that they have been out of print for some years showed how well they did what they were planned to do. Great progress has meanwhile been made in the geological survey of India, and a new edition, brought thoroughly up-to-date, was therefore much to be desired. The preparation of this was entrusted to Mr. R. D. Oldham; his "Bibliography of Indian Geology" (1888) suggested him as the most suitable man for the task, since a thorough acquaintance with the literature of the subject was one of the most essential qualifications. Mr. Oldham, moreover, has shown in numerous contributions to the *Records* and *Memoirs* of the Indian Survey, and in his recent sketch of "The Evolution of Indian Geography" in the *Geographical Journal* (March, 1894), that he is thoroughly in sympathy with the enquiries regarding the past physical geography of the country. And consideration of these is probably the only method of compiling a work that shall be interesting to read as well as valuable for reference.

In comparison with the former edition, the present has many advantages; it is issued as one volume instead of two; the scale of the map is reduced by about a third, and is, therefore, handier of reference, though there is more information in it; the long glossary is omitted; a most useful novelty is introduced in the index of localities, which occupies 32 pages; the latitude and longitude of each place is given, so that those unacquainted with the details of Indian topography can follow the descriptions with ease. The greatest improvement, however, is the abandonment of the system of describing the country in provinces; this method of treatment was no doubt inevitable in 1879, when the correlation of the rocks in the different regions was much less advanced than at present. The fact that description by formations instead of by provinces is now possible is one of the principal measures of the progress effected, which this volume attests. It is owing to this change that the work has been rendered more compact, for there is now one story instead of many.

In the list of contents Mr. Oldham has adopted the useful plan of printing in heavy type the titles of all those paragraphs which are either quite new or have been almost entirely rewritten. This, besides showing how much is to be credited to the original authors, directs attention to the subjects on which most work has been done in the interval between the two editions. Thus we see at once that little has been added to our knowledge of the pre-Palæozoic, the Palæozoic, the marine Jurassic, and the Cretaceous rocks of the Peninsula, or of the Deccan Traps. We note the absence of fresh information on the last of these with regret, for though Dr. Blanford's sketch of the subject (chap. xi.) is an admirable one, there are many points in the detailed stratigraphy, and especially in the petrography, on which further information is most desirable. In fact, in most of the Peninsular area, the facts remain much as they were, the principal additions being made to the knowledge of the correlation of the rocks with other areas in Asia, Africa, and Australia. Thus the description of the stratigraphy of the Gondwana system remains practically the same as before, though the chapter on the homotaxis of this formation is almost wholly new or rewritten. The fact that the great controversy as to the true correlation of this system is now at an end is noted with relief.

The principal changes in the volume are in chapters viii., xviii., and xix., which are entirely rewritten or new. The first is the one

already alluded to, dealing with the correlation of the Gondwana system. The other two summarise the evidence as to "The Age and Origin of the Himalayas," and give a sketch of the "Geological History of the Indian Peninsula." Attention is paid to the recent discussions upon the former, and the effort to prove that the Himalaya are of only very late Tertiary age, and that they are even now undergoing elevation. It is, however, maintained that the old view that the Himalaya are post-Eocene and pre-Pliocene is the correct one. The contentions of Middlemiss that the mountains were partially formed in pre-Tertiary times is also dismissed, but here the evidence is apparently less conclusive; the discussion of the subject leaves one with the impression that perhaps Mesozoic movements in the area were more important than has been believed. In discussing the formation of the Himalaya, attention is called to the Rev. Osmond Fisher's theory of mountain structure; a modification in this is proposed, and with this alteration the theory is said to find much support and illustration in the facts of Himalayan geology.

One of the most interesting points in which this edition differs from its predecessor is the different conclusion arrived at in respect to the formation of the Western Ghauts or Sahyadri Mountains. These are unlike the hills on the eastern side of the peninsula, which were grouped together in old geography books under the name of the Eastern Ghauts. For the Sahyadri Mountains have long been known to have a unity of structure, and to be of comparatively recent age. The hills on the Madras side of the peninsula, on the other hand, are not a single range, and were, with the Aravalli Mountains in Rajputana, among the oldest geological features in the country. In the 1879 edition the conclusion was maintained that the western scarps of the Sahyadri Mountains were formed by marine denudation, and were carried back some distance into the Tertiary rocks. Mr. Oldham, however, maintains that these mountains are the newest feature in Indian geography, and that their abrupt western scarps are the direct result of a dislocation. His arguments, moreover, seem absolutely conclusive as to the truth of this interesting and important view.

A charming photo-gravure of the eastern spur of the famous peak of Kanchanjanga (or Kichinjunga as it is probably better known in England) forms a frontispiece to the volume. This is so good that one wishes some more photographs of Indian scenery had been given in the book. We should have been glad to have seen some of the plates of fossils replaced by some of the photographs which have adorned recent volumes of the Indian Survey *Memoirs*. In a general book of this character, these would probably have been found of more general use and interest than figures of fossils.

Mr. Oldham is to be congratulated on the completion of his laborious task of revision, and we hope when a third edition is required the task will be placed in the hands of one who will do it with as much knowledge and originality.

J. W. G.

Amphioxus.

Amphioxus and the Ancestry of the Vertebrates. By A. Willey, B.Sc. Pp. xiv. and 316, with one plate and 135 illustrations, being Vol. II. of the Columbia University Biological Series. New York and London: Macmillan and Co. Price 10s. 6d. nett.

The literature of *Amphioxus* is not so large as that of many other groups; but it is nevertheless of a decidedly respectable bulk, particularly if various papers which bear indirectly upon the problems

which the structure of this animal gives rise to are included. Mr. Willey quotes no less than 133 papers which have been consulted by him in the course of preparing the volume now before us, and of these at least 100 may be fairly put down to *Amphioxus* itself. It is therefore a saving of much time to the hard-worked zoologist of the present day to have all this in a nut-shell—a cocoa-nut-shell possibly,—but still a work of moderate dimensions. Apart from this merit, the practice of writing a book about a definite animal of interest seems to us to be an excellent plan. It is a pity that not more is done along this line, which was really initiated by Mr. Huxley in his "Crayfish." The anatomy of birds, for example, might be woven into a volume upon such a type as *Apteryx*, one upon the Amphibia upon a plain account of the Axolotl, and so forth.

The present work contains everything that should be known about *Amphioxus*, besides a great deal that it is advantageous to know about the Tunicata, *Balanoglossus*, and some other types which come into structural relations with *Amphioxus*. *Amphioxus* was originally put down as a Mollusc, a fate which also overtook another equally remarkable and not less isolated type, *Peripatus*; the Mollusca in the early days of this century and the last days of the seventeenth appear to have been pretty well what the worms are now, a receptacle for anything unclaimed. The Russian naturalist Pallas, the discoverer of the "fish," was responsible for this placing of it; it was not until sixty years had elapsed that Costa redescribed *Amphioxus* and put it in its proper place, approximately at any rate. Nowadays there are really two parties which surge round *Amphioxus*; there are those who would look upon it as much degenerate and those who regard its simplicity as a mark of antiquity. There is no doubt much of both in the organisation of the animal. This "exquisite form," as Professor Osborn enthusiastically calls it in the preface which he contributes to Mr. Willey's book, is common in many parts of the world; it is, indeed, practically world-wide in range, or is represented by a considerable number of species which differ in but slight characters, the number of the myotomes being the chief means by which they are distinguished. Mr. Willey argues from its wide range and the trifling specific differences that the form is extremely ancient. We fully agree with Mr. Willey that the creature is in all probability an extremely archaic form, but his reasoning does not seem to us to prove the fact. In ancient forms it is surely more usual for the differences between species, or genera, as the case may be, to be great rather than small; intermediate forms have had time to die out, and long isolation has done its work. It is, rather, the newer groups that fade imperceptibly into each other. The very fact that there are so many diverse opinions about the classification of birds is indirect evidence of the comparative newness of the group; while the strikingly marked lines which divide from each other the several groups of reptiles are confirmatory of their age as shown by the rocks. The number of described species of *Amphioxus* is eight, but we believe that a ninth species was described at the recent meeting of the British Association at Oxford.

Amphioxus is, as will be inferred from what we have said concerning the bulk of the literature devoted to it, one of those animals which most naturalists have had dealings with. It is surprising, therefore, to learn how recently the excretory system has been discovered. For a long time the animal was believed to be without any specialised excretory organs of the vertebrate type—a piece of evidence which appeared to

indicate its degenerate character. But now, thanks to the investigations of several workers, we are in full possession of the details of the excretory system, which has been proved to be constituted on the plan of that of other Vertebrata, but, of course, with differences in details. The excretory tubes of the *Amphioxus* appear to represent the "head-kidney" or pronephros of other vertebrates. But they have no continuous longitudinal duct such as exists in all other types. The place of this is, however, physiologically taken by the atrial cavity, into which the series of tubes opens. It is now the general opinion that the longitudinal duct of the pronephros in the higher vertebrates is a formation from the epiblast—a groove to begin with, which is later converted into a tube; since the atrial cavity is also an epiblastic formation, no great gap is indicated by the absence of a special longitudinal duct.

It is, however, rather the comparisons with lower forms of life that are so interesting in *Amphioxus*. The connecting link between vetebrates and invertebrates has been long sought, and in spite of *Amphioxus*, Ascidian, *Balanoglossus*, or even the latest claimant, *Cephalodiscus*, it has not yet been found. For as all the animals which have been mentioned show the three essential Chordate characters, the dorsal nervous system, the notochord, and the gill-slits, we are really no further than we were before, and there is still hope for the Annelid to put in a claim which will be admitted. Mr. Willey gives, in his introductory remarks, an excellent *resumé* of the celebrated opinion of Geoffrey St. Hilaire upon the matter. The boldness—yet, so far as it went, logic—with which that great naturalist compared the exoskeleton of an insect with the ribs and vertebral column of a fish, have been equalled, if not surpassed—at any rate, so far as regards the boldness—by modern comparisons of Crustaceans and *Limulus* with vertebrates. Of the three essential chordate characters, the gill-slits would seem to be the hardest to account for; the existence of breathing-organs as mere processes of the skin seems so natural as it is very widely spread. There has, however, been a suggestion made, which is duly quoted by Mr. Willey, and which seems to shed at least a pin-point of light upon the matter. Mr. Harmer and Mr. Brooks have thrown out the hint that the function of the gill-slits was originally that of getting rid of the superfluous water; a form like *Cephalodiscus*, with copious arborescent gills, would appear to have no need of aërating the blood in the other, so to speak, far-fetched manner. And yet *Cephalodiscus* has gill-slits. The only invertebrates not within the charmed circle of the forms already referred to, which are seriously considered by Mr. Willey, are the Echinoderma and the Nemertines. The former group come in, of course, through the likeness of their larva to the *Tornaria* larva of *Balanoglossus*, as was fully explained in our October number, on p. 245 of this volume. The Nemertines have a proboscis sheath, which Professor Hubrecht compared to the notochord, and a pair of nerves which tend to become approximated dorsally, and therefore to occupy the position of the central nervous system of the vertebrate. Mr. Willey, however, at the conclusion of his book, feels unable to do more for the vertebrate pedigree than to suggest "that the proximate ancestor of the vertebrates was a free swimming animal, intermediate in organisation between an Ascidian tadpole and *Amphioxus*. . . . The ultimate or primordial ancestor of the Vertebrates would, on the contrary, be a worm-like animal, whose organisation was approximately on a level with that of the bilateral ancestors of the Echinoderms."

Index Kewensis.

Towards the end of October the third part of Jackson and Hooker's "Index Kewensis" appeared. This part ranges from *Kiblikia* to *Psidium*, and includes the large and difficult genera of grasses, *Paspalum*, *Panicum*, and *Poa*, the synonymy of which is most complicated, and for which there has been no guide beyond the unsatisfactory monograph of Steudel. The volume bears the signature "Vol. II.," and is paged 1–640. We do not see any advantage in this new pagination in the middle of a reference book, as it only causes trouble in quotation. The work has already been referred to at length in this Journal (Natural Science, iii., p. 379), and all we need do now is to express our admiration at the rapidity with which the Oxford University Press and Messrs. Jackson and Hooker are passing their proofs through the press.

The "Orient Guide."

This new edition of the "Orient Guide" is the fifth, the first having been issued in 1884. As each edition has consisted of from 10,000 to 15,000 copies, there must be a good many "Orient Guides" scattered about in the world. In the present issue a considerable number of alterations are to be found, some, but not many of them, being improvements. The opening chapter is no longer an account of the principal ships, but an essay by the newly-knighted Sir Wemyss Reid on ocean travelling in general. The medical chapter is omitted, as are Mrs. Henry Fawcett's delightful notes on Italy and Germany. Nearly as interesting was Mr. W. B. Richmond's account of Greece, of which only the Athens fragment remains. An account of the Australian colonies by Mr. Matthew Macfie has been added, and forms an attractive feature. Mr. Middleton Wake's exhaustive list of birds likely to be seen at sea is supplemented by a short description of Australian zoology. There seems to be some confusion on p. 257 between Bauer's parrakeet and Barnard's, whose name is spelt wrongly as Bernardius. There is quite a little treatise on the mariner's compass, a difficult subject, but clearly and simply explained by Commander Hull, R.N. The Rev. W. J. Loftie, the editor of the volume, must have had a laborious task in cutting and fitting together the varied mass of information of which we have given a brief epitome. He is to be congratulated upon the compact result.

Mr. J. W. Taylor is publishing a Monograph of the Land and Fresh Water Mollusca of the British Isles, of which the first part has just been received by us. It contains abundant illustration, and each part will be sold at the moderate cost of five shillings. The leading malacologists have been freely consulted in preparing the volume, which will doubtless be found of great use. In the circular which we have before us, some MS. opinions of eminent naturalists, such as the Rev. Canon Norman and the Rev. A. H. Cooke, are quoted. We hope to review the first volume when completed.

The first volume of the Cambridge Natural History is to appear, we are informed, either before or just about Christmas. This is to be upon the Mollusca, and is written by one of our foremost authorities on the group, the Rev. A. H. Cooke. The recent Brachiopoda are to be treated of by Mr. Shipley, one of the editors of the work; the fossil Brachiopoda by Mr. F. Cowper-Reed; the Polychaet worms by Dr. Benham; the Oligochaetes by Mr. Beddard, and other groups by other specialists.

Messrs. Swan Sonnenschein & Co. advertise a second edition of Mr. Beddard's "Animal Coloration," which appears to be a simple reprint without any further additions.

Some Recent Journals and Pamphlets.

On November 1 our contemporary *Nature* attained its twenty-fifth birthday, and started its fifty-first volume with a leader by Professor Huxley, in which he points out the remarkable effect of Darwinism on Natural Science. Professor Huxley wrote the first article in *Nature* when it first appeared as a weekly newspaper in 1870, and the present editor has just cause to congratulate himself on the fact that so distinguished a send-off has again been accorded to him.

Another contemporary, the *Feuille des Jeunes Naturalistes*, also proudly announces that it starts its twenty-fifth year of publication with the November 1 number. We congratulate Mr. Adrien Dollfus on the success which has crowned his energy and enthusiasm. Besides a paper by the editor on the Idoteidæ (Isopod crustacea) of the French coasts, there are others on the noxious insects of the Pines in Champagne by R. Hickel, and a long account, by H. Hua, of the recent Congress of the Botanical Society of France.

The Geological Society has just issued the fourth part of the fiftieth volume of its *Quarterly Journal*. It is a number of which any society might be proud, and forms a fitting conclusion to the fifty volumes that are to be indexed by the Society. The part consists of eleven papers, illustrated by sixteen plates, of which no less than six are exclusively palæontological, and all this quite apart from the "Additions to the Library," which forms the subject of a special paragraph in our "Notes and Comments." Of the papers themselves the most interesting, perhaps, are Dr. Gregory's observations on the Glacial Geology of Mt. Kenya, in which he shows that the glaciation has had a much greater extent formerly than now, and has in no inconsiderable manner determined the geographical distribution of animal and vegetable life. African geology is much to the fore, for there is a long paper by Captain Lyons on the Libyan desert, and two other papers by Mr. Draper on the Geology of South-Eastern Africa. Petrology is represented by papers on the older fragmental rocks of North-West Carnarvonshire, by Professor Bonney and Miss Raisin; on the structures of the Carboniferous Dolerites and Tuffs of Derbyshire, by H. H. Arnold-Bemrose; and on the banded structure of Tertiary Gabbros in Skye, by Sir A. Geikie and Mr. Teall. Some interesting plates accompany the two last-mentioned papers.

In Palæontology, Mr. Peach writes on additions to the fauna of the *Olenellus*-zone of the N.W. Highlands. This is a paper of the greatest interest and value. After a few words as to the locality, Mr. Peach describes the fossils, of which the following are new: *Olenellus reticulatus*, *O. gigas*, *O. intermedius*, and *Olenelloides*, a new genus with species *armatus*. Four beautiful plates by F. H. Michael illustrate the paper. The second palæontological contribution is that of Frederick Chapman on the Microzoa of the Bargate Stone, a series he has worked out, described with considerable care, and illustrated with two plates. Seven of the Ostracoda and eleven of the Foraminifera are described as new. Mr. Chapman notes that this comparatively large number of new forms is due to the fact that the Bargate series belong almost exclusively to the Laminarian and

Coralline zones of the old Neocomian Seas. The part concludes with a paper by Mr. Gresley on Cone-in-Cone structure, as it occurs in the Devonian series of Pennsylvania.

The September number of *The Journal of Marine Zoology and Microscopy* is in our hands, and appears to be fully up to the standard of other numbers of this useful periodical for the sea-side naturalist. There are four plates dealing respectively with the development of the *Balanus*, *Alcyonium*, and larval Ascidians, the anatomy of *Polynoe propinqua*, and the life-history of *Obelia geniculata*.

Dr. Packard sends us a paper, anatomical and systematic, upon the Siphonaptera, more generally known as Fleas (*Proc. Boston Soc. Nat. Hist.*, vol. xxvi.). The paper is largely a *resumé* of the work of others, but it contains some original matter. The fleas, like most other animals, have been tossed about in the system. The general opinion about them, however, at the present day is that they are closely allied to, if not forming one order with, the Diptera. Professor Packard puts them a little further away from the two-winged flies than many do. But he decidedly thinks that "they stand nearer to the Diptera than to any other order, and that they must have diverged from the ancestral dipterous stem before the existing forms of Diptera had become so extremely specialised as we now find them to be." The paper, which occupies 43 pages, is copiously illustrated by cuts.

Dr. A. G. Butler records, in the *Annals and Magazine of Natural History* for November, an amusing case of Walkerism—if we may so call it. It is the case of an unfortunate moth from Venezuela, which was described by Walker in vol. xv. of the British Museum Catalogue of Lepidoptera Heterocera under the name of *Celæna diffundens*. This same insect was described by the same author no less than six different times under six different specific names, and actually described under four of these names in one volume of the Catalogue (xxxiii.). As Dr. Butler truly says, "This kind of work needs no comment." But it is not an isolated instance.

In the *Geological Magazine* for November Dr. Henry Woodward describes an interesting series of Carboniferous Trilobites from the upper Limestones, Bank of the Hodder, near Stonyhurst, Lancashire. These include two new species of *Phillipsia* (*P. vandergrachti* and *P. polleni*) and the types have, by the generosity of the College authorities and the discoverers, been placed in the British Museum. The same number of the *Geological Magazine* contains a photograph of an erect tree, *Sigillaria*, in the Coal-measures of the valley of the Roach, Rochdale, contributed by Mr. J. C. Brierley and Sir Henry Howorth.

Dr. L. Rhumbler has a note in the *Zoologischer Anzeiger* (September) in which he shows that *Peneroplis* belongs to the perforate rather than to the imperforate Foraminifera.

Dr. Jentink has published a catalogue of the Crommelin collection of Dutch Birds, now in the Leyden Museum. Full records as to age, sex, and locality are given, and the book should therefore be of considerable value.

OBITUARY.

NATHANAEL PRINGSHEIM.

BORN 1823. DIED 1894.

THIS famous German botanist, whose death was recorded in our last number, was born in 1823 at Wziesko, in Upper Silesia. Starting as a medical student in the University of his native province, he afterwards went to Leipzig, and later to Berlin. In the meantime, however, he decided to devote himself to botany, and it was a botanical thesis in which his Ph.D. was gained at Berlin. After taking his degree he visited Paris, and on his return commenced his career, in 1851, at Berlin University. Five years after, at the age of 33, he was elected Member of the Berlin Academy of Sciences. In 1864 he was called to Jena to replace Schleiden as ordinary Professor, an appointment he held for four years. During his stay in Jena he founded an Institute of Plant-Physiology, the first of its kind. The idea took, similar laboratories were established at other Universities and agricultural high-schools, and a tremendous impetus was given to the study of plant-physiology. In 1868 Pringsheim returned to Berlin, but did not join the teaching staff of the University. From his own resources he founded a botanical laboratory, and soon established a school, to which belonged Strasburger, Tschirch, Vöchting, and other well-known botanists.

Pringsheim's great work was on the sexual relations in Cryptogams, especially Algæ, following up the results of Thuret and others. In a series of researches on the history of growth in individual families, he showed the existence in the Algæ of widely different forms of sexuality and development. Another subject attacked by Pringsheim was Chlorophyll. He was the first to demonstrate the true relation between the green colouring matter and the protoplasmic corpuscle; as to the function of chlorophyll, he held a view the reverse of that generally accepted, namely, that the colour acts as a shade, absorbing those rays of light which promote destructive metabolism, and thus making possible the constructive process.

A biographical notice would be incomplete without mention of the familiar "Jahrbücher für wissenschaftliche Botanik," started in 1857 and continued up to the present. Pringsheim was one of the oldest foreign members of the Linnean Society, having been elected on May 3, 1866.

PIERRE DUCHARTRE.

BORN 1811. DIED 1894.

THE eminent French botanist who has just died was Professor of Botany in the Faculty of Sciences at Paris, a member of the Institut, and also a foreign member of the Linnean Society of London, having been elected on May 3, 1877. He was the author of numerous papers on plant-biology, including observations on the flower and its organogeny, germination, and the young seedling, among which we may recall some valuable contributions on polycotyledonous embryos, respiration, transpiration, the question of the absorption of water by leaves, the vegetation of epiphytic plants and teratological notes. In 1867 he published a large and valuable textbook, entitled "Elemens de Botanique," of which a second edition appeared in 1877, and a third in 1885. In 1868 he produced an exhaustive report on the progress of plant-physiology. He was also responsible for the fourth volume of Jacques and Heurincq's "Manuel des Plantes," a systematically arranged encyclopædia of plants in cultivation, or of interest from an economic point of view. In his younger days he edited for two years (1845–1847) a monthly periodical, the *Revue Botanique*, a series of abstracts of the chief botanical works published at home and abroad. He was led to undertake this onerous piece of work by Benjamin Delessert, but, as many another young and enthusiastic scientific worker has found, it meant giving up a very large proportion of time, and stopped scientific research. The death of Delessert in 1847 was therefore made the pretext for the cessation of the *Revue*.

GUILLAUME LOUIS FIGUIER.

BORN FEBRUARY 15, 1819. DIED NOVEMBER 9, 1894.

THE death of this eminent French chemist and naturalist is announced. Figuier was born at Montpellier, at which town his father Jean was a chemist. He received his doctor's degree from the Faculty of Medicine of Montpellier, and left for Paris, where he worked in the laboratory of the Sorbonne, under Balard. In 1846 he became professor to the School of Medicine at Montpellier, where he remained five years, retiring to Paris in 1852. Since 1855 he had charge of the scientific columns of *La Presse*, and subsequently of *La France*. In 1856–57 he attacked the views of Claude Bernard regarding the secretion of sugar by the liver, and this at once brought him into prominence. About this time, too, Figuier resigned his appointments, renounced scientific research, and devoted himself entirely to the popularisation of science. In this he was conspicuously successful, and his popular books enjoyed a large and well-deserved circulation. His best-known works are "La Terre avant le Déluge," 1863; "La Terre et les Mers," 1864; "Histoire des Plantes," 1865; "Les Zoophytes et les Mollusques," 1866; "Les Poissons,

les Reptiles, et les Oiseaux," 1867; and a long series of similar publications. In one of his later works he was concerned to uphold the immortality of the soul. Most of his books have been translated into the chief European languages. When, however, he attempted to teach science from the stage of the Cluny Theatre by means of a play, entitled "Les Six Parties du Monde," he met with a less hearty welcome. Figuier died at Paris, aged 75 years.

The distinguished oculist and microscopist, Ludwig Mauthner, died at Vienna on October 20, at the comparatively early age of fifty-five. After studying at Vienna, he went to Berlin and studied under Albrecht von Gräfe. His famous work, "Lehrbuch der Ophthalmoscopie," appeared in 1868. He also published "Die Bestimmung der Refractionsanomalien mit Hilfe des Augenspiegels" in 1867, and "Vorlesen über die optischen Fehler des Auges" in 1876. He was part editor of the *Archiv für Augen- und Ohrenheit Kunde* for some years.

The well-known collector of Fossil Fishes, the Rev. Hugh Mitchell, LL.D., died at Aberdeen, about the middle of the month. His chief collections were made from the Old Red Sandstone of Forfarshire, and included the forms known as *Acanthodes mitchelli*, *Ischnacanthus gracilis*, and *Climatius scutiger*. Sir Philip Egerton, Mr. Powrie, and Professor Ray Lankester largely drew upon his collections when writing their important monographs. His collections are now in the British Museum of Natural History, having been purchased last year.

The death of Mr. Oscar Koechlin, of Alsace, which took place in September, robs that district of an enthusiastic entomologist, and the Mulhouse Society of one of its original and most energetic members. Dr. B. N. Rake, who was elected President of the Trinidad Field Naturalists' Club on August 3, in succession to Mr. H. Caracciolo, died on August 24 at Port-of-Spain. Mr. Henry Tate, the acting Government analyst of the same island, also fell a victim to the prevailing malignant fever on August 18.

The news from Africa during the past month is very serious. Telegrams have appeared which record the deaths of the German botanist Dr. Lent, and the zoologist Kretzschmar, in the Kilimanjaro district, killed by natives, and of Mr. G. L. E. Andreæ, a member of a Belgian trading company, at Mukikamu, near Stanley Pool. Mr. Andreæ was an entomologist, and several parcels of insects have been sent by him from Africa.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Dr. William Peterson has been appointed Principal of the McGill University, in the room of Sir William Dawson. Principal Dawson resigned about a year ago. Dr. Peterson has been for twelve years head of University College, Dundee.

Dr. P. L. Sclater has been elected an Honorary Fellow of Corpus Christi College, Oxford. He was formerly a Fellow of this, his old college.

Sir Thomas Temple, of Ballintemple, Wicklow, succeeds to the Vice-Presidency of the Royal Dublin Society. The office was rendered vacant by the death of the Right Hon. William Cogan.

Dr. Carl v. Heider, formerly an assistant in the Zoological Institute of Berlin University, has been appointed Professor of Zoology at the University of Innsbruck, and Dr. Michael von Lenhossek, Professor of Anatomy. Dr. von Lenhossek comes from Würzburg. Dr. R. von Lendenfeld has become Professor of Zoology in the University of Czernowitz; and Dr. Schewiakov leaves Heidelberg and goes to the Imperial Academy of St. Petersburg as assistant in the Zoological Institute.

Mr. and Mrs. Theodore Bent have started on another expedition to Southern Arabia, where they hope to continue the archæological and botanical work commenced last year in the Hadramaut Valley. They expect to return to England next April.

Mr. A. Trevor-Battye, about whose safety so unnecessary a fuss was made in October last, has returned from Kolguev Island, where he went about the end of June to study and collect the birds of the island.

We regret to learn that M. de Brazza, the well-known explorer, suffered shipwreck near Tchumber, on his way back from Kounde to Brazzaville on September 30. M. de Brazza was no worse for his experience, but all his papers were unfortunately lost.

A Life of Joseph Wolfe, the famous painter of animals, has just been completed by Mr. A. H. Palmer. Many examples of his work are in the possession of the Zoological Society, some of the most striking of which are those of the larger apes, and hang on the walls of the meeting room.

Mr. Spencer Moore, who, under somewhat unfavourable conditions, recently made a successful botanical exploration of a portion of the Matto Grosso district in Brazil, has gone to seek a better return for his labour in the gold-fields of West Australia. We trust he will make money enough for him to devote the rest of his life to botanical work.

Science at Oxford University will lose a friend in Professor Sir Henry W. Acland, who has stated that he will resign the Regius Professorship of Medicine at the close of the year. He has fought the cause of Science for many years at Oxford in the teeth of much opposition, and has carefully promoted and watched over the development of museums and educational matters in the University. But no doubt his withdrawal from the cares of office will not altogether deprive his colleagues of his valuable aid.

Cambridge University benefits under the will of the late Mr. Samuel Sandars to the extent of £2,000. The money is left for the endowment of a reader in Bibliography.

In our third volume (p. 451) we published an account of the proposed Sedgwick Memorial Museum at Cambridge, by Mr. H. Woods. We regret to learn from the *Cambridge University Reporter* that "even on the lowest tender, the cost of the building would exceed by £4,500 the estimate which was before the Senate when the Grace approving the plan was passed." Since it is considered that the University cannot afford so large a sum, the Syndicate appointed to report on the matter have recommended that the plan be reconsidered, and if necessary a new one substituted for it.

Among the examiners for the Natural Science Tripos at Cambridge will be found Professor Lewis and H. A. Miers for Mineralogy; Philip Lake and Professor Cole for Geology; W. Bateson and Professor Hickson for Zoology; Professor Oliver and W. Gardiner for Botany, while for Human Anatomy Professor Macalister and Dr. Rolleston, and for Physiology W. B. Hardy and Professor Schäfer have been appointed.

From the Report of the Director of the Science and Art Museum, Dublin, we learn that the electric light will be extended to the Natural History Museum and various other buildings during the current year. We wish the same could be said of our London institution. One of the most valuable additions made to the collections last year was the collection of British Marine and Land Mollusca formed by Edward Waller. This collection contains a large number of the rarities belonging to Gwyn Jeffreys, whose own collection was unfortunately allowed to go to America. A small timber museum is being prepared, and arrangements are being made for the exhibition of illustrative sets of Irish plants from various localities. The Botanical laboratory is now in good going order, and original investigations on Irish Marine Algæ are in course of progress by Miss Hensman. The attendance of the public at the Museum is in excess of previous years, and the daily average was 1,000. With regard to Sunday attendance the average for the Museum was 979, and that for the Botanic Gardens 5,386. This is most encouraging. The Director insists on the importance of keeping up the library, the grants being insufficient for the purpose of the purchase of books necessary for the determination of the specimens. This is a want also keenly felt in our London institutions.

We regret that the Raffles Museum at Singapore is still without a curator. It will be remembered that about a year ago, Dr. Haviland, who had come from the Sarawak Museum in Borneo, and who had done much to improve the condition of the Raffles Museum and to save money to its Committee, was compelled to resign on account of the insufficiency of his salary, only 200 dollars (£20 16s.) a month, which the Government refused to increase. This valuable museum has since then been in the charge of a clerk, at 150 dollars a month, nor is there any immediate prospect of a more competent individual being appointed. We regret the financial position of the Colony, but hope that it will not be thought necessary to extend this penny-wise policy to other of its scientific appointments.

On the completion of the decorations and renovation to which we have already referred, the Bristol Museum was re-opened by the municipal authorities on November 12. The hours are now extended from 10 a.m. to 9 p.m. daily. A Reference Library has been added. An extension of the Oldham Public Library and Museum has also been opened this month, in the form of a lecture-hall capable of seating 900 people. Free lectures are given each Saturday evening during the winter. The Art Gallery at Oldham is already famous among Lancashire institutions, and it is now proposed to devote attention to providing an adequate Natural History Collection.

The Royal Society Medals have been awarded as follows :—Professor Cleve, of Upsala, receives the Davy medal for his researches on the rare earths; Professor Huxley, the Darwin Medal; Professor Victor Horsley, one of the Royal Medals. No more appropriate award could be given to Professor Huxley, and we are glad to hear that Professor Horsley is to have a Royal Medal for researches so beneficial to mankind, though mankind is already sufficiently grateful to so distinguished a benefactor.

We learn from the *Birmingham Post* that the card catalogue in the library of the Royal College of Surgeons will be utilised, when finished, as copy for a new catalogue of the library. A printed catalogue of this rich medical library would be of great service, as many books dealing with general natural science are contained therein. The College has also acquired a ground plan of John Hunter's house in Leicester Square, drawn by William Clift, Hunter's pupil, which shows the arrangement, not only of his rooms, but much of their contents.

At the opening meeting of the Zoological Society, held on November 6, Professor Sir William Flower exhibited and read extracts from a letter and diaries from Emin Pasha. The letter recorded the dispatch of specimens for the British Museum, but was undated. The diary commenced on May 29, 1892, and was continued till October 12, 1892, when Emin was close to Stanley Falls. It consisted chiefly of notes on the birds he had collected on this his last journey. The collections have never been received in this country, and it is probable they were destroyed by the band that murdered the traveller. The manuscripts had been received by Sir William Flower from the agent of the Congo State at Brussels.

The Geologists' Association inaugurated their session on November 2 by an exhibition of lantern slides of photographs of places of geological interest, instead of holding the usual conversazione. The exhibition, which seemed to be of much interest to the members, was chiefly remarkable for some views taken in deep mines in Cornwall, by Mr. W. Thomas, and others of the Festiniog district, taken by Mr. J. C. Burrows. The views were confined to British subjects, and Messrs. Preston, Lardeur, and Hiddon showed excellent results taken during the various excursions of the Association.

At the next meeting of the Association, on December 7, Mr. A. Smith Woodward promises a note on the Megalosaurian teeth that have been discovered by Mr. J. Alstone in the Portlandian of Aylesbury, after which (the Honorary Secretary asks us to state) Professor McKenny Hughes, Mr. H. W. Monckton, and Dr. Frazer Hume will describe the geology of their recent tour in Switzerland.

From the *Journal* of the Oxford University Scientific Club, of which no. 18 has just reached us, we learn that Mr. M. D. Hill has been granted the use of the British Association Table at Naples, where he intends to continue his researches on artificial fertilisation. At a meeting of the Club on October 26, Mr. Garstang read a paper "On some modifications of the Tunicate pharynx induced by the violent ejection of water."

Despite the exertions of this energetic Club, we regret to observe that the study of geology still thrives so ill in this same University of Oxford. The examiners for

the Burdett-Coutts' Scholarship have once more had to report that no candidate of sufficient merit has presented himself.

The *Journal of the Marine Biological Association* (vol. iii., no. 3) contains the statements of receipts and expenditure for the year ending May 31, 1894, as well as the directors' report. The income for the past year was £2,172, including the annual grants of £1,000 from H.M. Treasury and £400 from the Fishmongers' Company. In connection with our Editorial note in the last number, it is interesting to read that the rent of tables produced only £36 16s. 3d. We are glad to hear that the trouble about bad sea-water supply has been detected and satisfactorily settled.

The *American Naturalist* states that the American Association for the Advancement of Science has again subscribed $100 for a table at the Marine Biological Laboratory at Woods Holl. Any member or applicant for membership of the Association may apply for the table through the permanent secretary, Professor F. W. Putnam, Cambridge, Mass. The award is made by a committee of five.

A "Botanical Society of America" has been founded in Brooklyn, N.Y., and Professor W. Trelease, of St. Louis, has been chosen first president.

The A.W.P.L. and P.N.E.U. bid fair to become as firmly established among us as the Y.M.C.A. and the M.A.B.Y.S. The A.W.P.L. however has, in consequence possibly of the rivalry of the Pioneer Club, changed its name to the "Women Lecturers' Association," and will no doubt be symbolised by W.L.A. Under the auspices of the W.L.A. a course of six lectures for children on "Our Animal Friends" is being delivered by Mrs. Rose (Certified Student, Royal College of Science, London). The time is 3 p.m. on Wednesdays, and the place is, as heretofore, one of the geological galleries in the Natural History Museum. Tables of strata and cases of rocks and fossil shells are so very appropriate to talks on the Cat, Dog, Horse, Rabbit, and Mouse. The kindness of the trustees also permits the Museum specimens to be drawn upon for the illustration of the lectures. Tickets for the course are 10s. Our representative informed us that the first two lectures were poorly attended. We hope the rest will be more successful.

Meanwhile the Belgravia branch of the P.N.E.U. announces certain free lectures, one of which, on December 4, should at least attract those interested in natural science. It is "On the study of Natural History as a means to Moral and Mental Development," and will be delivered by Miss Shackleton, at 35 Cranley Gardens, S.W., at 4.30 p.m. The Secretary of the P.N.E.U. is Miss Jennie Paterson, 28 Victoria Street, S.W. The Secretary of the W.L.A. is Miss Edith Bradley, 4 Caroline Place, W.C.

A course of twenty-five lectures on the History of Geography and of Geographical Discovery, by Mr. H. J. Mackinder, is now in progress at Gresham College on Mondays at 6 p.m. These lectures are under the auspices of the Royal Geographical Society and the London University Extension.

The interesting series of Cantor Lectures, which were delivered by Mr. Hugh Stanners on "Artificial Foliage in Architecture," are now being printed in the *Journal of the Society of Arts*. The stem and the flower are dealt with in the *Journal* for November 2. They are accompanied by some good illustrations.

We learn from the *Pall Mall Gazette* that Mr. Hovey, in the course of a paper read before the American Forestry Society, stated that the remains of silicified trees in Arizona, known as the "Arizona fossil forest," are being ground into powder by a commercial company as a substitute for emery.

CORRESPONDENCE.

"ANLAGEN."

THE word "blast," proposed (NATURAL SCIENCE, v., p. 368) as an equivalent for the German "Anlage," has the certainly great merit of shortness, but has it not likewise some shocking demerits in the way of earlier associations? We are accustomed indeed to the blast of a bugle or a trumpet; but what is to be thought about "the blast of an organ"? I venture to suggest "primule" as an irreproachable term.

Salt Hill, Galway. W. E. HART.

THE MUSEUMS' ASSOCIATION.

As a member of the Museums' Association I read the note entitled "Science at a Picnic," in your August number, with much interest, not unmixed with astonishment at your boldness. Now that I have returned from a distant land where even NATURAL SCIENCE did not penetrate, I have read the replies from the Secretaries of the Association, and from a member of the Dublin Local Committee, to which your strictures not unnaturally gave rise. Now it would be affectation on my part to overlook the fact that I am the member of the Association alluded to by Messrs. Platnauer, Howarth, and Johnson, who appeared at the last moment with "some notes," "of an extremely discursive nature," "which he wished to descant upon," etc., etc. If this paper of mine—which certainly was discursive in that it dealt with fifteen museums in different parts of the world—is one of those to which you alluded as having been burked, I beg leave to state that you are in error. I had absolutely no *locus standi*. Not only did I send no intimation of my paper till after the programme was printed, but though I had received most courteous permission by telegraph to read the paper on the first day, I did not appear with it till the last day; and in spite of this I met with the utmost consideration at the hands of the officers of the Association, of various members of the local committee, and especially of Professor Johnson himself. I may also add that, although my paper is one of great length, it has been most cordially accepted for publication in the Report of the Association.

After saying this, I do not think I can possibly be misunderstood if I venture to express my agreement with your main thesis. Without referring to what was privately said by other unofficial members of the Association, I need only state that I, at least, was one of those who "could ill spare their time," and to whom, consequently, "the insertion of a day's excursion between the two days of meeting proved vexatious." This, indeed, was the very reason why I was unable to intimate my intention of attending the meeting; and after all I only managed to be present by travelling two nights in succession. It is perfectly possible, as Professor Johnson says, that discussion "was invited once or twice, and was not forthcoming." People who have the most to say are not likely to speak when the allotted time has already been exceeded Silence is especially golden when the luncheon hour has struck and much business has yet to be got through. It would be invidious to mention special papers, and obviously one cannot give the names of members who undoubtedly would have spoken on them had time permitted. It is enough to contrast the statement of the Secretaries that "the volume of *Proceedings* of the Dublin Meeting will be the largest that has ever been published by the Association," with the other statement, also by the Secretaries, that only seven hours "were devoted to papers and discussions. F. A. BATHER.

[Our Comments on this subject were written with full knowledge of the circumstances attending Mr. Bather's oration. We did not count it among the papers of which the reading and discussion were unduly curtailed.—ED.]

I hope you will permit me a word in this discussion. As a member of the Association who was present throughout the Dublin meeting, although I personally feel extremely grateful to the able Secretaries and to the local committee, whose indefatigable exertions made the meeting the success it was, I must nevertheless express sympathy with the feeling which actuated your editorial note in Natural Science for August. The object of our Association is undoubtedly purely practical; that is, we meet mainly for the purpose of seeing and learning of new and efficient mechanical appliances and new methods, for the display and arrangement of the objects in Museums, considered entirely apart from the scientific or artistic details of the objects themselves. This we aim at accomplishing by the inspection of the Museums within reach of the place of meeting, by the reading of papers which describe the arrangements, actual or proposed, in these or other Museums, and by the discussion and explanation of the same. Now, on the last day of the meeting I did feel that the reading of papers and the discussion were considerably restricted owing to want of time; and one kept silence in regard to many points on which one would have liked more information, because this want was evident.

The excursion to Bray—which all those who joined in it described (so far as I heard) as charming (for I personally employed the day in re-visiting the Museums in Dublin, gathering details of the many excellent devices introduced there by Dr. Ball and his officers, some of which I am now trying to give effect to here)—was a part of the programme that some others besides myself felt might have been postponed, and the day given instead to the discussion of the many important appliances described, and other matters raised (but *scarcely* settled) in papers read before the Association. Lord Powerscourt's collection, though of the highest interest *per se*, did not pretend to any arrangement which could afford new hints to the *Museum Curator*. Such luxuries as this excursion and visits to other places of great interest in themselves, but only indirectly beneficial to us as *Curators*, might, on future occasions, I think, be advantageously deferred till the true work of the Association has been accomplished, when those who are able to remain would enjoy them with greater zest and a clear conscience.

The Museums, Liverpool. Henry O. Forbes.

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